[illegible] 1813.

# MEDICAL ESSAYS

AND

# OBSERVATIONS.

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MEDICAL

# MEDICAL ESSAYS AND OBSERVATIONS.

## ARTICLE I.

### *The Meteorological Register.*

THE instruments described in Art. II. of our first volume, were employed in making the following Observations.

## *JUNE* 1733.

| D. | Hour. | Baro. In. | Baro. D. | Ther. In. | Ther. D. | Hyg. I. | Hyg. D. | Wind. Dir. | Wind. For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 9 | 12 | 7 | 0 | 9 | N | 2 | fair | |
| | 8 p m | 29 | 8 | 13 | 3 | 0 | 9 | W | 3 | fair | |
| 2 | 9 a m | 29 | 8 | 13 | 7 | 1 | 1 | W by N | 2 | fair | |
| | 8 p m | 29 | 9 | 14 | 2 | 1 | 5 | N by W | 1 | fair | |
| 3 | 9 a m | 30 | 0 | 12 | 6 | 1 | 3 | E | 1 | cloudy | |
| | 7 p m | 30 | 0 | 13 | 4 | 1 | 1 | E | 1 | cloudy | |
| 4 | 9 a m | 30 | 0 | 13 | 6 | 1 | 1 | S E | 1 | cloudy | |
| | 8 p m | 29 | 9 | 13 | 0 | 1 | 5 | E | 0 | cloudy | |
| 5 | 8 a m | 29 | 8 | 13 | 3 | 1 | 8 | E | 2 | cloudy | |
| | 7 p m | 29 | 8 | 12 | 6 | 1 | 5 | E | 2 | cloudy | |
| 6 | 9 a m | 29 | 8 | 13 | 4 | 1 | 5 | E | 1 | fair | |
| | 7 p m | 29 | 8 | 12 | 8 | 1 | 5 | E | 2 | fair | |
| 7 | 9 a m | 29 | 8 | 13 | 4 | 1 | 5 | E | 1 | fair | |
| | 7 p m | 29 | 8 | 15 | 0 | 1 | 2 | E | 1 | cloudy | |
| 8 | 9 a m | 29 | 7 | 12 | 9 | 1 | 5 | E | 2 | cloudy | |
| | 6 p m | 29 | 8 | 13 | 0 | 1 | 3 | E | 2 | fair | |
| 9 | 9 a m | 29 | 7 | 13 | 3 | 1 | 2 | E | 1 | fair | |
| | 7 p m | 29 | 6 | 13 | 2 | 1 | 4 | S E | 1 | cloudy | |
| 10 | 9 a m | 29 | 8 | 12 | 7 | 1 | 2 | N E | 1 | fair | |
| | 7 p m | 29 | 9 | 12 | 7 | 1 | 2 | E | 2 | cloudy | |
| 11 | 9 a m | 29 | 9 | 13 | 8 | 1 | 4 | E | 1 | fair | |
| | 7 p m | 29 | 9 | 13 | 7 | 1 | 3 | E | 0 | cloudy | |
| 12 | 9 a m | 29 | 9 | 14 | 8 | 1 | 5 | S E | 0 | cloudy | 0,034 |
| | 8 p m | 29 | 7 | 13 | 5 | 1 | 8 | E | 2 | rain | |
| 13 | 9 a m | 29 | 7 | 13 | 4 | 2 | 8 | N | 0 | rain | |
| | 5 p m | 29 | 7 | 14 | 5 | 2 | 1 | N | 1 | cloudy | |
| 14 | 9 a m | 29 | 7 | 14 | 4 | 2 | 1 | N | 1 | fair | 0,386 |
| | 8 p m | 29 | 7 | 15 | 3 | 1 | 4 | W | 1 | fair | |
| 15 | 9 a m | 29 | 8 | 15 | 6 | 1 | 4 | S W | 1 | cloudy | 0,350 |
| | 8 p m | 29 | 8 | 15 | 5 | 1 | 1 | S W | 1 | cloudy | |
| 16 | 9 a m | 29 | 7 | 16 | 0 | 1 | 5 | S | 0 | cloudy | 0,190 |
| | 8 p m | 29 | 6 | 14 | 3 | 1 | 4 | S W | 2 | fair | |
| | | | | | | | | | | | 0,960 |

# *JUNE* 1733.

| D. | Hour. | Baro In | D. | Ther. In | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,960 |
| 17 | 9 a m | 29 | 6 | 15 | 6 | 1 | 2 | S | 2 | cloudy | 0,085 |
| | 8 p m | 29 | 5 | 14 | 3 | 1 | 4 | S W | 2 | cloudy | |
| 18 | 9 a m | 29 | 5 | 14 | 6 | 1 | 6 | W | 2 | cloudy | 0,055 |
| | 8 p m | 29 | 3 | 3 | 5 | 1 | 4 | W | 2 | cloudy | |
| 19 | 9 a m | 29 | 2 | 4 | 1 | 1 | 4 | W | 2 | fair | 0,035 |
| | 9 p m | 29 | 1 | 13 | 5 | 1 | 5 | S W | 1 | rain | |
| 20 | 9 a m | 28 | 9 | 14 | 1 | 1 | 8 | E | 1 | rain | 0,316 |
| | 5 p m | 28 | 9 | 14 | 5 | 1 | 5 | W | 2 | fair | |
| 21 | 9 a m | 29 | 3 | 13 | 9 | 1 | 7 | W | 3 | fair | 0,173 |
| | 5 p m | 29 | 6 | 3 | 7 | 1 | 3 | W | 3 | fair | |
| 22 | 9 a m | 29 | 8 | 4 | 2 | 1 | 4 | S W | 1 | fair | 0,145 |
| | 5 p m | 29 | 9 | 14 | 6 | 1 | 3 | S W | 1 | cloudy | |
| 23 | 9 a m | 29 | 9 | 15 | 0 | 1 | 5 | W | 2 | cloudy | |
| | 8 p m | 29 | 9 | 15 | 2 | 1 | 2 | W | 0 | fair | |
| 24 | 9 a m | 29 | 9 | 14 | 9 | 1 | 3 | E | 1 | fair | |
| | 8 p m | 29 | 9 | 15 | 4 | 1 | 5 | [illegible] | 0 | fair | |
| 25 | 9 a m | 29 | 8 | 16 | 0 | 1 | 4 | S W | 0 | cloudy | 0,084 |
| | 9 p m | 29 | 9 | 16 | 0 | 1 | 3 | W | 0 | fair | |
| 26 | 9 a m | 29 | 9 | 15 | 3 | 1 | 7 | E | 0 | fog | 0,100 |
| | 7 p m | 29 | 9 | 16 | 1 | 1 | 7 | E | 0 | fair | |
| 27 | 9 a m | 29 | 9 | 15 | 2 | 1 | 8 | E | 1 | fog | |
| | 8 p m | 29 | 8 | 14 | 1 | 2 | 5 | E | 2 | rain | |
| 28 | 9 a m | 29 | 8 | 16 | 0 | 1 | 6 | S W | 2 | cloudy | 0,150 |
| | 7 p m | 29 | 8 | 15 | 0 | [illegible] | 2 | S W | 1 | cloudy | |
| 29 | 9 a m | 29 | 8 | 14 | 8 | 1 | 4 | S W | 2 | fair | |
| | 8 p m | 29 | 8 | 14 | 1 | 1 | 7 | S W | 2 | cloudy | |
| 30 | 9 a m | 29 | 9 | 14 | 7 | 1 | 6 | S W | 2 | fair | 0,035 |
| | 8 p m | 29 | 9 | 14 | 7 | 1 | 6 | S W | 2 | fair | |
| H. at a med. | | 29 | 8 | 14 | 2 | 1 | 3 | | | Total depth | 2,138 |
| Gr. height | | 30 | 0 | 16 | 1 | 2 | 8 | | | | |
| L. height | | 28 | 9 | 12 | 6 | 0 | 9 | | | | |

## *JULY* 1733.

| D. | Hour. | Baro. In. | Baro. D. | Ther. In | Ther. D. | Hyg. I. | Hyg. D. | Wind. Dir. | Wind. For. | Weather. | Rain. In. D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 9 | 15 | 5 | 1 | 4 | N W | 1 | fair | |
| | 8 p m | 29 | 9 | 15 | 0 | 1 | 1 | W | 1 | fair | |
| 2 | 9 a m | 29 | 9 | 15 | 3 | 1 | 5 | E | 1 | fair | |
| | 8 p m | 29 | 9 | 15 | 8 | 1 | 3 | E | 1 | fair | |
| 3 | 9 a m | 29 | 9 | 14 | 5 | 1 | 4 | E | 1 | fair | |
| | 7 p m | 30 | 0 | 15 | 0 | 1 | 5 | E | 1 | fair | |
| 4 | 9 a m | 30 | 1 | 14 | 7 | 1 | 9 | E | 1 | fair | |
| | 5 p m | 30 | 0 | 15 | 1 | 1 | 7 | E | 1 | fair | |
| 5 | 9 a m | 29 | 9 | 15 | 7 | 1 | 7 | S E | 1 | fair | |
| | 5 p m | 29 | 9 | 15 | 6 | 1 | 6 | W | 2 | cloudy | 0,045 |
| 6 | 9 a m | 30 | 0 | 15 | 0 | 1 | 5 | S E | 2 | fair | |
| | 5 p m | 30 | 0 | 15 | 7 | 1 | 5 | S W | 2 | fair | |
| 7 | 8 a m | 30 | 1 | 14 | 5 | 1 | 5 | W | 2 | cloudy | |
| | 9 p m | 30 | 0 | 5 | 6 | 1 | 6 | W | 0 | fair | |
| 8 | 9 a m | 29 | 9 | 15 | 1 | | 4 | W | 3 | fair | |
| | 8 p m | 29 | 9 | 14 | 2 | 1 | 4 | W | 2 | cloudy | |
| 9 | 9 a m | 29 | 7 | 14 | 3 | 1 | 6 | W | 3 | cloudy | |
| | 5 p m | 29 | 7 | 14 | 3 | 1 | 6 | W | 3 | cloudy | |
| 10 | 9 a m | 29 | 7 | 13 | 8 | 1 | 3 | N W | 3 | fair | |
| | 8 p m | 29 | 8 | 3 | 5 | 1 | 2 | N W | 3 | cloudy | 0 145 |
| 11 | 9 a m | 29 | 8 | 14 | 2 | 1 | 5 | W | 3 | fair | |
| | 5 p m | 29 | 8 | 14 | 9 | 1 | 5 | W | 3 | cloudy | |
| 12 | 9 a m | 29 | 8 | 13 | 9 | 1 | 6 | W | 2 | cloudy | |
| | 5 p m | 29 | 7 | 15 | 0 | 1 | 6 | W | 1 | cloudy | |
| 13 | 9 a m | 29 | 6 | 14 | 5 | 1 | 7 | W | 2 | fair | |
| | 5 p m | 29 | 5 | 5 | 3 | 1 | 4 | W | 3 | fair | |
| 14 | 9 a m | 29 | 6 | 12 | 2 | 2 | 1 | N E | 2 | cloudy | 0,093 |
| | 7 p m | 29 | 6 | 13 | 2 | 1 | 4 | N E | 1 | fair | |
| 15 | 9 a m | 29 | 6 | 14 | 6 | 1 | 4 | N E | 1 | fair | |
| | 8 p m | 29 | 6 | 13 | 7 | 1 | 3 | N E | 1 | fair | |
| 16 | 8 a m | 29 | 6 | 13 | 1 | 2 | 2 | E | 2 | rain | |
| | 5 p m | 29 | 6 | 12 | 7 | 2 | 8 | N E | 2 | cloudy | |
| | | | | | | | | | | | 0,[illegible]83 |

# *JULY* 1733.

| D. | Hour. | Baro. In | Baro. D. | Ther. In | Ther. D. | Hyg I. | Hyg D. | Wind. Dir. | Wind. For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,283 |
| 17 | 8 a m | 29 | 6 | 13 | 4 | 3 | 5 | N E | 2 | cloudy | |
| | 4 p m | 29 | 6 | 14 | 0 | 2 | 0 | N E | 1 | cloudy} | |
| 18 | 9 a m | 29 | 7 | 14 | 1 | 2 | 0 | N | 1 | fair | 0,054 |
| | 5 p m | 29 | 7 | 14 | 0 | 1 | 6 | N E | 0 | fair | |
| 19 | 8 a m | 29 | 7 | 13 | 6 | 1 | 6 | N W | 2 | fair | |
| | 8 p m | 29 | 7 | 14 | 1 | 1 | 7 | N W | 2 | fair | |
| 20 | 9 a m | 29 | 8 | 14 | 0 | 1 | 6 | W | 2 | fair | |
| | 5 p m | 29 | 8 | 15 | 4 | 1 | 4 | W | 2 | fair | |
| 21 | 8 a m | 29 | 7 | 14 | 0 | 2 | 0 | W | 2 | fair | |
| | 7 p m | 29 | 7 | 15 | 6 | 1 | 2 | N W | 1 | fair | |
| 22 | 9 a m | 29 | 8 | 15 | 5 | 1 | 5 | W | 2 | fair | |
| | 6 p m | 29 | 8 | 15 | 5 | 1 | 3 | W | 2 | fair | |
| 23 | 9 a m | 29 | 8 | 14 | 9 | 1 | 6 | W | 2 | cloudy | |
| | 5 p m | 29 | 8 | 15 | 8 | 1 | 6 | W | 3 | cloudy | |
| 24 | 9 a m | 29 | 8 | 15 | 1 | 2 | 0 | W | 2 | fair | |
| | 5 p m | 29 | 8 | 15 | 3 | 1 | 4 | W | 2 | fair | |
| 25 | 8 a m | 29 | 9 | 14 | 0 | 1 | 7 | W | 2 | fair | 0,115 |
| | 5 p m | 29 | 9 | 14 | 8 | 1 | 3 | W | 3 | fair | |
| 26 | 9 a m | 29 | 8 | 14 | 4 | 1 | 5 | W | 3 | fair | |
| | 8 p m | 29 | 8 | 14 | 0 | 1 | 5 | W | 2 | fair | |
| 27 | 8 a m | 29 | 8 | 13 | 3 | 1 | 6 | W | 2 | fair | |
| | 5 p m | 29 | 8 | 14 | 8 | 1 | 0 | W | 2 | fair | |
| 28 | 9 a m | 29 | 7 | 14 | 7 | 1 | 4 | S | 1 | fog | |
| | 4 p m | 29 | 6 | 15 | 3 | 1 | 3 | S by W | 0 | cloudy | |
| 29 | 9 a m | 29 | 5 | 14 | 3 | 1 | 5 | E | 1 | fair | |
| | 7 p m | 29 | 4 | 14 | 3 | 1 | 9 | W | 1 | cloudy | |
| 30 | 8 a m | 29 | 4 | 13 | 8 | 2 | 0 | N by W | 1 | fair | 0,186 |
| | 5 p m | 29 | 4 | 14 | 5 | 1 | 1 | N W | 2 | fair | |
| 31 | 8 a m | 29 | 4 | 13 | 0 | 1 | 7 | W | 2 | fair | |
| | 7 p m | 29 | 4 | 13 | 2 | 1 | 4 | N W | 2 | fair | |
| H. at a med. | | 29 | 7 | 14 | 6 | 1 | 6 | | | Total depth | 0,638 |
| Gr. height | | 30 | 1 | 15 | 8 | 3 | 5 | | | | |
| L. height | | 29 | 4 | 12 | 2 | 1 | 0 | | | | |

## *AUGUST* 1733.

| D. | Hour. | Baro. In. | Baro. D. | Ther. In. | Ther. D. | Hyg. I. | Hyg. D. | Wind. Dir | Wind. For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 5 | 13 | 5 | 1 | 3 | W | 2 | cloudy | |
| | 6 p m | 29 | 6 | 13 | 9 | 1 | 3 | N W | 1 | fair | |
| 2 | 9 a m | 29 | 6 | 12 | 7 | 1 | 5 | W | 1 | fair | |
| | 8 p m | 29 | 6 | 13 | 8 | 1 | 0 | E | 1 | fair | |
| 3 | 9 a m | 29 | 6 | 13 | 6 | 1 | 3 | E | 1 | fair | |
| | 8 p m | 29 | 5 | 13 | 5 | 1 | 4 | E | 1 | fair | |
| 4 | 9 a m | 29 | 4 | 13 | 3 | 1 | 5 | S E | 2 | cloudy | |
| | 8 p m | 29 | 5 | 13 | 6 | 1 | 5 | E | 1 | cloudy | |
| 5 | 9 a m | 29 | 5 | 13 | 8 | 1 | 8 | E | 1 | cloudy | |
| | 8 p m | 29 | 5 | 13 | 5 | 1 | 8 | E | 1 | rain | |
| 6 | 9 a m | 29 | 4 | 14 | 4 | 2 | 4 | E | 1 | rain | 0 056 |
| | 8 p m | 29 | 4 | 13 | 3 | 3 | 1 | E | 1 | fog | |
| 7 | 9 a m | 29 | 4 | 13 | 7 | 3 | 1 | E | 2 | cloudy | 0,093 |
| | 7 p m | 29 | 4 | 13 | 6 | 3 | 2 | N E | 1 | fog | |
| 8 | 9 a m | 29 | 4 | 13 | 6 | 3 | 3 | N E | 1 | cloudy | |
| | 8 p m | 29 | 5 | 13 | 6 | 2 | 2 | W | 1 | cloudy | |
| 9 | 9 a m | 29 | 7 | 13 | 1 | 1 | 9 | N W | 0 | cloudy | |
| | 5 p m | 29 | 7 | 14 | 8 | 1 | 5 | N W | 0 | fair | |
| 10 | 9 a m | 29 | 7 | 13 | 6 | 1 | 8 | S W | 0 | cloudy | 0,075 |
| | 7 p m | 29 | 7 | 13 | 4 | 1 | 9 | S E | 1 | cloudy | |
| 11 | 8 a m | 29 | 7 | 12 | 4 | 2 | 0 | W | 1 | fair | |
| | 6 p m | 29 | 8 | 14 | 5 | 1 | 3 | W | 1 | fair | |
| 12 | 9 a m | 29 | 7 | 15 | 0 | 1 | 4 | S W | 4 | cloudy | 0,035 |
| | 6 p m | 29 | 7 | 13 | 5 | 1 | 3 | S W | 4 | cloudy | |
| 13 | 8 a m | 29 | 7 | 13 | 1 | 1 | 4 | W | 2 | fair | |
| | 5 p m | 29 | 8 | 14 | 3 | 1 | 2 | W | 1 | fair | |
| 14 | 9 a m | 29 | 4 | 14 | 1 | 1 | 3 | S W | 3 | fair | 0,188 |
| | 4 p m | 29 | 4 | 13 | 8 | 1 | 4 | W | 3 | fair | |
| 15 | 9 a m | 29 | 7 | 12 | 4 | 1 | 7 | W | 1 | fair | 0,210 |
| | 5 p m | 29 | 7 | 14 | 0 | 1 | 4 | W | 1 | cloudy | |
| 16 | 9 a m | 29 | 6 | 12 | 0 | 1 | 8 | N W | 1 | rain | 0,490 |
| | 4 p m | 29 | 7 | 13 | 0 | 1 | 7 | N W | 0 | cloudy | |
| | | | | | | | | | | | 1,147 |

## *AUGUST* 1733.

| D. | Hour. | Baro. In | D. | Ther. In | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 1,147 |
| 17 | 9 a m | 29 | 7 | 12 | 4 | 1 | 6 | S W | 2 | fair | 0,365 |
| | 5 p m | 29 | 4 | 13 | 2 | 1 | 6 | S W | 4 | cloudy | |
| 18 | 9 a m | 29 | 3 | 13 | 6 | 2 | 0 | S W | 3 | cloudy | |
| | 5 p m | 29 | 2 | 13 | 4 | 1 | 8 | S W | 3 | cloudy | |
| 19 | 9 a m | 29 | 7 | 12 | 5 | 1 | 5 | N W | 2 | cloudy | 0,154 |
| | 7 p m | 29 | 8 | 13 | 0 | 1 | 3 | N W | 0 | cloudy | |
| 20 | 8 a m | 29 | 7 | 13 | 8 | 1 | 7 | S W | 0 | rain | |
| | 5 p m | 29 | 6 | 15 | 0 | 1 | 9 | S W | 1 | cloudy | |
| 21 | 8 a m | 29 | 6 | 14 | 1 | 2 | 3 | S W | 2 | cloudy | |
| | 7 p m | 29 | 6 | 12 | 9 | 1 | 9 | W | 1 | cloudy | |
| 22 | 8 a m | 29 | 7 | 12 | 6 | 1 | 9 | W | 1 | fair | |
| | 7 p m | 29 | 7 | 12 | 9 | 1 | 5 | W | 1 | fair | |
| 23 | 9 a m | 29 | 8 | 12 | 7 | 1 | 8 | W | 1 | fair | 0,193 |
| | 7 p m | 29 | 8 | 13 | 2 | 1 | 4 | W | 1 | fair | |
| 24 | 9 a m | 29 | 7 | 13 | 2 | 1 | 4 | S W | 2 | cloudy | 0,055 |
| | 6 p m | 29 | 5 | 13 | 9 | 1 | 5 | S W | 3 | fair | |
| 25 | 9 a m | 29 | 5 | 12 | 3 | 1 | 5 | W | 2 | fair | |
| | 7 p m | 29 | 6 | 12 | 3 | 1 | 5 | W | 2 | fair | |
| 26 | 9 a m | 29 | 6 | 12 | 9 | 1 | 6 | N W | 2 | fair | |
| | 6 p m | 29 | 7 | 13 | 2 | 1 | 2 | N W | 1 | fair | |
| 27 | 9 a m | 29 | 6 | 12 | 8 | 1 | 9 | S W | 1 | cloudy | 0,100 |
| | 7 p m | 29 | 6 | 12 | 8 | 1 | 6 | W | 1 | fair | |
| 28 | 9 a m | 29 | 5 | 11 | 5 | 1 | 6 | W | 1 | fair | |
| | 6 p m | 29 | 4 | 12 | 7 | 1 | 6 | S | 0 | cloudy | |
| 29 | 9 a m | 29 | 1 | 12 | 1 | 1 | 9 | S W | 1 | rain | 0,286 |
| | 5 p m | 29 | 1 | 12 | 6 | 1 | 4 | W | 1 | fair | 0,375 |
| 30 | 9 a m | 29 | 4 | 12 | 5 | 1 | 5 | N W | 2 | fair | |
| | 5 p m | 29 | 5 | 13 | 0 | 1 | 3 | N W | 1 | fair | |
| 31 | 9 a m | 29 | 3 | 12 | 1 | 1 | 9 | S | 1 | rain | |
| | 6 p m | 29 | 2 | 12 | 4 | 1 | 4 | W | 2 | fair | |
| H. at a med. | | 29 | 6 | 13 | 2 | 1 | 7 | | | Total depth | 2,675 |
| Gr. height | | 29 | 8 | 15 | 0 | 3 | 3 | | | | |
| L. height | | 29 | 1 | 12 | 1 | 1 | 0 | | | | |

## SEPTEMBER 1733.

| D. | Hour. | Baro. In | D. | Ther. In | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 2 | 12 | 6 | 1 | 7 | S W | 2 | fair | 0,084 |
| | 6 p m | 29 | 0 | 11 | 5 | 1 | 5 | W | 2 | cloudy | |
| 2 | 9 a m | 28 | 9 | 12 | 7 | 1 | 5 | S W | 1 | cloudy | 0,055 |
| | 6 p m | 29 | 0 | 12 | 6 | 1 | 6 | E | 1 | fair | |
| 3 | 8 a m | 29 | 2 | 12 | 0 | 1 | 7 | N W | 2 | cloudy | |
| | 4 p m | 29 | 4 | 12 | 8 | 1 | 2 | N W | 2 | fair | |
| 4 | 9 a m | 29 | 5 | 12 | 0 | 1 | 7 | S W | 3 | fair | 0,230 |
| | 4 p m | 29 | 2 | 11 | 5 | 1 | 5 | S W | 2 | rain | |
| 5 | 9 a m | 29 | 0 | 12 | 2 | 1 | 8 | S W | 3 | fair | 0,067 |
| | 5 p m | 29 | 3 | 12 | 4 | 1 | 5 | W | 3 | fair | |
| 6 | 8 a m | 29 | 6 | 11 | 3 | 2 | 0 | W | 2 | fair | |
| | 6 p m | 29 | 7 | 12 | 3 | 1 | 4 | S W | 2 | fair | |
| 7 | 8 a m | 29 | 6 | 11 | 4 | 1 | 6 | W | 2 | fair | |
| | 5 p m | 29 | 7 | 12 | 1 | 1 | 5 | W | 2 | fair | |
| 8 | 9 a m | 29 | 8 | 11 | 0 | 1 | 9 | W | 2 | fair | 0,108 |
| | 5 p m | 29 | 9 | 11 | 0 | 1 | 5 | N | 2 | cloudy | |
| 9 | 9 a m | 30 | 0 | 10 | 5 | 1 | 6 | N | 2 | fair | |
| | [illegible] p m | 30 | 1 | 12 | 1 | 1 | 3 | N by W | 1 | fair | |
| 10 | [illegible] a m | 30 | 1 | 11 | 3 | 1 | 5 | S W | 1 | fair | |
| | [illegible] p m | 30 | 1 | 12 | 3 | 1 | 3 | S by E | 1 | fair | |
| 11 | [illegible] a m | 30 | 1 | 11 | 3 | 1 | 5 | S by E | 2 | fair | |
| | 4 p m | 30 | 0 | 13 | 5 | 1 | 2 | S by E | 2 | fair | |
| 12 | 8 a m | 29 | 8 | 11 | 9 | 1 | 7 | S E | 2 | fair | 0,077 |
| | [illegible] p m | 29 | 7 | 13 | 4 | 1 | 6 | S E | 2 | rain | |
| 13 | [illegible] a m | 29 | 8 | 13 | 3 | 1 | 9 | S | 1 | rain | 0,030 |
| | [illegible] p m | 29 | 8 | 14 | 3 | 1 | 8 | S | 1 | cloudy | |
| 14 | 9 a m | 29 | 9 | 13 | 0 | 3 | 0 | S E | 1 | mist | 0,0[illegible]6 |
| | 5 p m | 29 | 9 | 13 | 4 | 2 | 8 | S E | 1 | mist | |
| 15 | 9 a m | 30 | 0 | 12 | 5 | 3 | 3 | S E | 1 | mist | |
| | 5 p m | 30 | 0 | 12 | 3 | 3 | 2 | E | 2 | mist | |
| 16 | 9 a m | 30 | 0 | 12 | 2 | 2 | 7 | E | 1 | fog | |
| | 5 p m | 30 | 0 | 11 | 3 | 2 | 1 | E | 1 | fair | |
| | | | | | | | | | | | 0,677 |

## *SEPTEMBER* 1733.

| D. | Hour. | Baro. In | D. | Ther. In. | D. | Hyg. I. | D. | Wind Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,677 |
| 17 | 9 a m | 30 | 1 | 12 | 0 | 2 | 1 | E | 1 | fog | |
| | 5 p m | 30 | 1 | 13 | 0 | 1 | 9 | E | 1 | cloudy | |
| 18 | 8 a m | 30 | 2 | 12 | 3 | 2 | 1 | S W | 1 | cloudy | |
| | 5 p m | 30 | 2 | 13 | 0 | 2 | 1 | S W | 1 | cloudy | |
| 19 | 9 a m | 30 | 2 | 13 | 2 | 2 | 4 | S W | 2 | fair | |
| | 5 p m | 30 | 1 | 13 | 8 | 1 | 7 | W | 2 | fair | |
| 20 | 9 a m | 29 | 9 | 13 | 6 | 1 | 9 | S W | 2 | cloudy | |
| | 4 p m | 29 | 9 | 13 | 8 | 2 | 0 | S W | 2 | rain | |
| 21 | 9 a m | 29 | 8 | 11 | 9 | 2 | 2 | W by S | 2 | fair | 0,356 |
| | 6 p m | 29 | 9 | 11 | 7 | 2 | 0 | S W | 2 | fair | |
| 22 | 9 a m | 29 | 9 | 12 | 2 | 2 | 5 | W | 2 | fair | |
| | 5 p m | 29 | 9 | 12 | 8 | 1 | 9 | W | 1 | fair | |
| 23 | 9 a m | 29 | 8 | 12 | 5 | 2 | 0 | S W | 1 | fair | 0,240 |
| | 4 p m | 29 | 7 | 13 | 1 | 1 | 6 | S W | 1 | fair | |
| 24 | 9 a m | 29 | 4 | 12 | 5 | 2 | 2 | S W | 2 | fair | |
| | 5 p m | 29 | 3 | 13 | 2 | 1 | 7 | S | 2 | lowring | |
| 25 | 9 a m | 29 | 1 | 11 | 0 | 1 | 9 | S W | 2 | fair | |
| | 5 p m | 28 | 9 | 11 | 8 | 1 | 8 | S by W | 2 | rain | |
| 26 | 9 a m | 28 | 4 | 10 | 8 | 2 | 0 | S W | 4 | cloudy | 0,395 |
| | 5 p m | 28 | 6 | 10 | 5 | 1 | 7 | S W | 3 | cloudy | |
| 27 | 9 a m | 28 | 9 | 11 | 1 | 1 | 8 | W | 3 | fair | 0,094 |
| | 4 p m | 29 | 2 | 11 | 8 | 1 | 5 | W | 3 | fair | |
| 28 | 9 a m | 29 | 1 | 12 | 3 | 2 | 1 | S W | 2 | cloudy | |
| | 5 p m | 29 | 1 | 12 | 3 | 1 | 7 | S W | 4 | cloudy | |
| 29 | 9 a m | 29 | 4 | 11 | 7 | 1 | 8 | W | 4 | cloudy | |
| | 5 p m | 29 | 5 | 11 | 6 | 1 | 7 | W | 3 | fair | 0,073 |
| 30 | 9 a m | 29 | 7 | 11 | 4 | 1 | 9 | W | 2 | fair | |
| | 5 p m | 29 | 7 | 11 | 1 | 1 | 7 | W | 1 | cloudy | |
| H. at a med. | | 29 | 6 | 12 | 2 | 1 | 9 | | | Total depth | 1,835 |
| Gr. height | | 30 | 2 | 13 | 8 | 3 | 3 | | | | |
| L. height | | 28 | 4 | 10 | 5 | 1 | 2 | | | | |

## *OCTOBER* 1733.

| D. | Hour | Baro. In. | Baro. D. | Ther. In. | Ther. D. | Hyg. I. | Hyg. D. | Wind. Dir. | Wind. For. | Weather. | Rain. In. D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 8 | 11 | 2 | 2 | 5 | S W | 1 | rain | 0,045 |
| | 5 p m | 29 | 5 | 12 | 4 | 1 | 9 | S W | 1 | cloudy | |
| 2 | 9 a m | 29 | 5 | 13 | 3 | 2 | 1 | S W | 1 | cloudy | |
| | 5 p m | 29 | 4 | 13 | 7 | 1 | 8 | S W | 3 | cloudy | |
| 3 | 9 a m | 29 | 1 | 12 | 9 | 1 | 7 | S W | 4 | cloudy | 0,06[illegible] |
| | 5 p m | 29 | 1 | 12 | 5 | 1 | 7 | S W | 4 | fair | |
| 4 | 9 a m | 29 | 5 | 11 | 6 | 2 | 0 | W | 2 | fair | |
| | 5 p m | 29 | 7 | 11 | 8 | 1 | 5 | N W | 2 | fair | |
| 5 | 9 a m | 30 | 1 | 10 | 6 | 1 | 3 | W by N | 1 | fair | |
| | 4 p m | 30 | 2 | 11 | 0 | 1 | 4 | W by N | 1 | fair | |
| 6 | 9 a m | 30 | 3 | 9 | 9 | 1 | 7 | N W | 1 | fair | |
| | 5 p m | 30 | 4 | 10 | 4 | 1 | 7 | N W | 0 | fair | |
| 7 | 9 a m | 30 | 4 | 10 | 5 | 1 | 8 | S W | 1 | cloudy | |
| | 4 p m | 30 | 2 | 11 | 3 | 1 | 7 | S W | 1 | cloudy | |
| 8 | 9 a m | 30 | 0 | 11 | 4 | 1 | 7 | S W | 1 | fair | |
| | 5 p m | 30 | 0 | 11 | 7 | 2 | 7 | W by S | 2 | cloudy | |
| 9 | 9 a m | 30 | 0 | 11 | 4 | 2 | 6 | N E | 2 | rain | |
| | 5 p m | 30 | 0 | 10 | 9 | 2 | 7 | E | 2 | fog | |
| 10 | 9 a m | 29 | 9 | 11 | 1 | 2 | 4 | S E | 1 | cloudy | |
| | 5 p m | 29 | 8 | 11 | 4 | 2 | 2 | S E | 2 | cloudy | 0,124 |
| 11 | 9 a m | 29 | 7 | 11 | 3 | 2 | 3 | S E | 1 | cloudy | |
| | 5 p m | 29 | 6 | 11 | 3 | 2 | 0 | E | 2 | cloudy | |
| 12 | 9 a m | 29 | 5 | 11 | 2 | 2 | 4 | S E | 1 | lowring | |
| | 5 p m | 29 | 4 | 11 | 3 | 2 | 3 | E | 1 | cloudy | |
| 13 | 9 a m | 29 | 4 | 9 | 1 | 2 | 2 | N W | 2 | fair | |
| | 5 p m | 29 | 5 | 10 | 7 | 1 | 1 | N W | 3 | cloudy | |
| 14 | 9 a m | 29 | 7 | 10 | 2 | 1 | 9 | N W | 2 | fair | |
| | 5 p m | 29 | 8 | 10 | 4 | 1 | 5 | N W | 2 | fair | |
| 15 | 9 a m | 30 | 0 | 9 | 4 | 1 | 7 | N W | 2 | fair | |
| | 5 p m | 30 | 0 | 10 | 2 | 1 | 7 | N W | 2 | fair | |
| 16 | 9 a m | 30 | 1 | 10 | 1 | 2 | 0 | N W | 1 | fair | |
| | 5 p m | 30 | 2 | 10 | 8 | 1 | 7 | N W | 1 | fair | |
| | | | | | | | | | | | 0,2[illegible] |

## OCTOBER 1733.

| D. | Hour. | Baro. In. | D. | Ther. In. | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,229 |
| 17 | 9 a m | 30 | 3 | 8 | 9 | 1 | 9 | W by N | 1 | fair | |
| | 5 p m | 30 | 3 | 9 | 4 | 1 | 7 | W by N | 1 | fair | |
| 18 | 9 a m | 30 | 4 | 9 | 9 | 1 | 9 | S W | 1 | fair | |
| | 4 p m | 30 | 4 | 11 | 0 | 1 | 8 | S | 1 | fair | |
| 19 | 9 a m | 30 | 4 | 10 | 8 | 1 | 9 | S W | 1 | fair | |
| | 4 p m | 30 | 4 | 11 | 5 | 1 | 8 | W | 0 | fair | |
| 20 | 9 a m | 30 | 3 | 10 | 0 | 1 | 8 | W | 2 | fair | 0,030 |
| | 4 p m | 30 | 3 | 11 | 0 | 1 | 7 | W | 2 | fair | |
| 21 | 9 a m | 30 | 3 | 10 | 6 | 1 | 7 | W | 0 | cloudy | |
| | 4 p m | 30 | 2 | 11 | 1 | 1 | 7 | W | 2 | cloudy | |
| 22 | 9 a m | 30 | 1 | 10 | 7 | 2 | 0 | S W | 2 | fair | |
| | 5 p m | 30 | 1 | 10 | 8 | 2 | 0 | W | 2 | fair | |
| 23 | 9 a m | 30 | 0 | 10 | 6 | 2 | 0 | W | 0 | cloudy | |
| | 4 p m | 29 | 9 | 11 | 0 | 2 | 0 | W | 0 | cloudy | |
| 24 | 9 a m | 29 | 7 | 10 | 1 | 2 | 0 | S W | 0 | fair | |
| | 4 p m | 29 | 5 | 10 | 8 | 1 | 7 | S W | 2 | cloudy | |
| 25 | 9 a m | 29 | 0 | 11 | 5 | 2 | 2 | S W | 2 | cloudy | |
| | 4 p m | 28 | 9 | 11 | 9 | 1 | 7 | S W | 2 | cloudy | |
| 26 | 9 a m | 29 | 0 | 10 | 8 | 1 | 9 | S W | 3 | fair | |
| | 4 p m | 29 | 1 | 11 | 2 | 2 | 0 | S W | 3 | cloudy | 0,206 |
| 27 | 9 a m | 29 | 1 | 10 | 1 | 1 | 9 | S W | 3 | cloudy | |
| | 4 p m | 29 | 2 | 9 | 8 | 1 | 8 | S W | 3 | rain | 0,154 |
| 8 | 9 a m | 29 | 4 | 10 | 3 | 2 | 0 | S W | 2 | cloudy | |
| | 4 p m | 29 | 3 | 11 | 9 | 2 | 4 | S W | 3 | rain | 0,380 |
| 29 | 9 a m | 29 | 1 | 11 | 7 | 2 | 4 | S W | 4 | cloudy | |
| | 4 p m | 29 | 1 | 10 | 6 | 1 | 9 | W | 4 | cloudy | 0,084 |
| 30 | 9 a m | 29 | 7 | 9 | 0 | 1 | 6 | W | 2 | cloudy | |
| | 4 p m | 30 | 0 | 9 | 2 | 1 | 4 | N W | 2 | cloudy | |
| 31 | 9 a m | 30 | 2 | 9 | 3 | 1 | 5 | S W | 2 | cloudy | |
| | 5 p m | 29 | 9 | 11 | 3 | 1 | 9 | S W | 3 | cloudy | |
| H. at a med. | | 29 | 8 | 10 | 9 | 1 | 9 | | | Total depth | 1,083 |
| Gr. height | | 30 | 4 | 13 | 7 | 2 | 7 | | | | |
| L. height | | 28 | 9 | 8 | 9 | 1 | 3 | | | | |

## *NOVEMBER* 1733.

| D. | Hour. | Baro. In. D. | Ther. In. D. | Hyg. I. D. | Wind. Dir. | For. | Weather. | Rain. In. D. |
|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 7 | 12 2 | 2 3 | S W | 3 | cloudy | |
| | 4 p m | 29 7 | 12 4 | 2 3 | S W | 2 | cloudy | |
| 2 | 9 a m | 29 7 | 12 4 | 2 3 | W | 3 | fair | 0,043 |
| | 5 p m | 29 7 | 11 8 | 2 2 | W | 3 | cloudy | |
| 3 | 9 a m | 29 8 | 10 6 | 2 8 | W | 2 | cloudy | |
| | 4 p m | 29 9 | 10 2 | 1 7 | W | 2 | fair | |
| 4 | 9 a m | 30 3 | 9 4 | 1 8 | N | 1 | fair | |
| | 4 p m | 30 3 | 10 0 | 1 7 | N | 1 | cloudy | |
| 5 | 9 a m | 30 1 | 11 6 | 2 5 | N | 3 | cloudy | |
| | 4 p m | 30 1 | 12 0 | 2 3 | W | 2 | cloudy | |
| 6 | 9 a m | 30 2 | 11 8 | 2 3 | W | 0 | cloudy | |
| | 4 p m | 30 3 | 11 9 | 2 2 | W | 1 | cloudy | |
| 7 | 9 a m | 30 3 | 11 5 | 2 0 | W | 1 | cloudy | 0,085 |
| | 5 p m | 30 2 | 11 8 | 1 7 | W | 1 | cloudy | |
| 8 | 9 a m | 30 0 | 11 4 | 2 0 | S | 0 | cloudy | |
| | 5 p m | 29 9 | 11 5 | 2 0 | S | 3 | cloudy | |
| 9 | 9 a m | 29 8 | 10 8 | 2 2 | S W | 2 | fair | |
| | 4 p m | 29 7 | 10 3 | 1 9 | S W | 2 | fair | |
| 10 | 9 a m | 29 2 | 10 7 | 1 8 | S W | 3 | fair | |
| | 4 p m | 29 1 | 9 9 | 1 9 | S W | 4 | cloudy | |
| 11 | 9 a m | 29 2 | 9 1 | 2 0 | W | 2 | fair | |
| | 4 p m | 29 2 | 9 7 | 2 0 | W | 2 | cloudy | |
| 12 | 9 a m | 29 2 | 8 2 | 2 0 | N W | 2 | fair | |
| | 4 p m | 29 5 | 9 4 | 1 9 | N W | 2 | fair | |
| 13 | 9 a m | 29 7 | 9 3 | 2 0 | S W | 2 | cloudy | 0,066 |
| | 4 p m | 29 6 | 10 8 | 2 0 | S W | 2 | cloudy | |
| 14 | 9 a m | 29 4 | 12 0 | 2 0 | S W | 3 | cloudy | |
| | 5 p m | 29 6 | 13 5 | 2 0 | S W | 2 | cloudy | |
| 15 | 9 a m | 29 6 | 12 4 | 2 3 | S W | 2 | cloudy | |
| | 5 p m | 29 5 | 12 7 | 2 0 | S W | 4 | cloudy | |
| 16 | 9 a m | 29 8 | 9 8 | 2 0 | S W | 2 | fair | |
| | 5 p m | 29 8 | 9 8 | 1 9 | S W | 2 | fair | |
| | | | | | | | | 0,194 |

## *NOVEMBER* 1733.

| D. | Hour. | Baro. In | D. | Ther. In | D. | Hyg I. | D. | Wind Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,194 |
| 17 | 9 a m | 29 | 5 | 10 | 6 | 2 | 0 | S | 4 | cloudy | |
| | 5 p m | 29 | 6 | 9 | 9 | 1 | 9 | W | 2 | cloudy | |
| 18 | 9 a m | 29 | 8 | 9 | 7 | 2 | 0 | S W | 2 | fair | |
| | 4 p m | 29 | 8 | 10 | 4 | 1 | 9 | S W | 2 | fair | |
| 19 | 9 a m | 29 | 7 | 9 | 6 | 2 | 0 | S W | 2 | fair | |
| | 4 p m | 29 | 8 | 10 | 4 | 2 | 0 | S W | 2 | fair | |
| 20 | 9 a m | 29 | 7 | 11 | 0 | 2 | 1 | S | 2 | cloudy | |
| | 4 p m | 29 | 6 | 10 | 6 | 1 | 9 | S | 2 | cloudy | |
| 21 | 9 a m | 29 | 5 | 10 | 9 | 2 | 0 | S E | 2 | fair | |
| | 4 p m | 29 | 6 | 11 | 3 | 2 | 0 | S E | 2 | fair | |
| 22 | 9 a m | 29 | 8 | 11 | 7 | 2 | 1 | S | 1 | fair | |
| | 4 p m | 29 | 8 | 11 | 9 | 2 | 0 | S | 1 | fair | |
| 23 | 9 a m | 29 | 8 | 11 | 5 | 2 | 1 | S W | 2 | fair | |
| | 4 p m | 29 | 7 | 11 | 5 | 2 | 0 | S W | 3 | cloudy | |
| 24 | 9 a m | 29 | 7 | 11 | 9 | 2 | 7 | S W | 2 | cloudy | |
| | 4 p m | 29 | 6 | 12 | 4 | 2 | 4 | S W | 2 | cloudy | |
| 25 | 9 a m | 29 | 7 | 10 | 2 | 1 | 9 | W | 3 | fair | 0,037 |
| | 4 p m | 26 | 7 | 10 | 3 | 1 | 9 | W | 3 | fair | |
| 26 | 9 a m | 29 | 7 | 10 | 2 | 2 | 0 | W | 3 | fair | |
| | 4 p m | 29 | 5 | 9 | 4 | 2 | 5 | W | 4 | rain | |
| 27 | 9 a m | 29 | 8 | 9 | 5 | 1 | 8 | W | 2 | fair | |
| | 4 p m | 29 | 9 | 10 | 2 | 1 | 8 | W | 2 | cloudy | |
| 28 | 9 a m | 29 | 8 | 11 | 0 | 2 | 4 | W | 2 | fair | |
| | 4 p m | 29 | 7 | 11 | 1 | 1 | 9 | W | 3 | cloudy | |
| 29 | 9 a m | 29 | 8 | 11 | 0 | 2 | 0 | W | 2 | cloudy | |
| | 4 p m | 29 | 7 | 11 | 8 | 2 | 2 | W | 3 | rain | 0,095 |
| 30 | 9 a m | 29 | 5 | 11 | 5 | 2 | 0 | S W | 3 | fair | |
| | 4 p m | 29 | 6 | 9 | 7 | 1 | 9 | S W | 3 | cloudy | |
| H at a med. | | 29 | 7 | 10 | 8 | 2 | 0 | | | Total depth 0,326 | |
| Gr. height | | 30 | 3 | 12 | 7 | 2 | 7 | | | | |
| L. height | | 29 | 1 | 8 | 2 | 1 | 7 | | | | |

## *DECEMBER* 1733.

| D. | Hour. | Baro. In. | Baro. D. | Ther. In. | Ther. D. | Hyg. I. | Hyg. D. | Wind. Dir. | Wind. For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 6 | 9 | 3 | 2 | 0 | W | 2 | fair | 0,205 |
| | 4 p m | 29 | 6 | 9 | 2 | 2 | 0 | W | 2 | fair | |
| 2 | 9 a m | 29 | 4 | 12 | 0 | 2 | 6 | W | 4 | cloudy | 0,146 |
| | 4 p m | 29 | 3 | 11 | 1 | 2 | 1 | W | 4 | fair | |
| 3 | 9 a m | 29 | 7 | 9 | 6 | 1 | 7 | W | 2 | fair | 0,072 |
| | 4 p m | 29 | 9 | 10 | 1 | 1 | 9 | W | 2 | fair | |
| 4 | 9 a m | 30 | 0 | 11 | 2 | 2 | 2 | W | 2 | cloudy | |
| | 4 p m | 30 | 1 | 11 | 4 | 2 | 3 | W | 2 | fair | |
| 5 | 9 a m | 30 | 2 | 11 | 1 | 2 | 1 | W | 1 | cloudy | |
| | 4 p m | 30 | 2 | 11 | 4 | 2 | 1 | S W | 2 | cloudy | |
| 6 | 9 a m | 30 | 2 | 10 | 7 | 2 | 0 | S W | 2 | cloudy | |
| | 4 p m | 30 | 1 | 11 | 1 | 1 | 9 | S W | 1 | cloudy | |
| 7 | 9 a m | 30 | 0 | 10 | 0 | 2 | 0 | S W | 1 | cloudy | |
| | 4 p m | 30 | 0 | 10 | 2 | 1 | 9 | S W | 1 | cloudy | |
| 8 | 9 a m | 30 | 0 | 9 | 7 | 2 | 0 | S W | 1 | fair | |
| | 4 p m | 29 | 9 | 10 | 7 | 2 | 0 | S W | 1 | fair | |
| 9 | 9 a m | 29 | 7 | 9 | 0 | 1 | 9 | S W | 1 | fog | |
| | 4 p m | 29 | 6 | 9 | 5 | 1 | 9 | S W | 1 | cloudy | |
| 10 | 9 a m | 29 | 1 | 10 | 5 | 2 | 2 | S | 2 | cloudy | |
| | 4 p m | 29 | 3 | 10 | 2 | 2 | 1 | S W | 2 | cloudy | |
| 11 | 9 a m | 29 | 0 | 11 | 5 | 2 | 4 | S W | 4 | rain | 0, 48 |
| | 4 p m | 29 | 0 | 12 | 0 | 2 | 2 | S W | 4 | rain | |
| 12 | 9 a m | 29 | 1 | 10 | 7 | 1 | 9 | S | 1 | fair | 0,153 |
| | 4 p m | 29 | 0 | 11 | 8 | 2 | 1 | S | 1 | fair | |
| 13 | 9 a m | 29 | 1 | 10 | 7 | 2 | 2 | S | 0 | fog | 0,124 |
| | 4 p m | 29 | 0 | 10 | 8 | 2 | 1 | S W | 2 | cloudy | |
| 14 | 9 a m | 29 | 0 | 10 | 9 | 2 | 0 | S | 2 | fog | 0,176 |
| | 4 p m | 28 | 8 | 11 | 4 | 2 | 2 | S | 2 | cloudy | |
| 15 | 9 a m | 29 | 2 | 11 | 1 | 2 | 1 | S W | 3 | cloudy | 0,198 |
| | 4 p m | 29 | 3 | 10 | 9 | 1 | 9 | S W | 4 | cloudy | |
| 16 | 9 a m | 29 | 2 | 10 | 3 | 2 | 2 | S | 2 | rain | 0,354 |
| | 4 p m | 29 | 3 | 9 | 9 | 2 | 3 | N W | 1 | fair | |
| | | | | | | | | | | | 1,666 |

## *DECEMBER* 1733.

| D | Hour | Baro. In | Baro. D. | Ther. In | Ther. D. | Hyg I. | Hyg D. | Wind. Dir. | Wind. For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 1,666 |
| 17 | 9 a m | 29 | 4 | 10 | 0 | 2 | 4 | W | 2 | rain | 0,565 |
| | 4 p m | 29 | 4 | 10 | 3 | 2 | 5 | W | 1 | fair | |
| 18 | 9 a m | 29 | 4 | 12 | 6 | 2 | 3 | S W | 4 | fair | 0,223 |
| | 4 p m | 29 | 4 | 12 | 5 | 2 | 5 | S W | 3 | fair | |
| 19 | 9 a m | 29 | 6 | 11 | 5 | 2 | 2 | S W | 2 | fair | |
| | 4 p m | 29 | 7 | 11 | 7 | 2 | 2 | W | 1 | fair | |
| 20 | 9 a m | 29 | 7 | 11 | 7 | 2 | 2 | S E | 1 | cloudy | 0,274 |
| | 4 p m | 29 | 7 | 11 | 7 | 2 | 2 | S W | 1 | cloudy | |
| 21 | 9 a m | 29 | 6 | 11 | 9 | 2 | 3 | S | 0 | fog | 0,074 |
| | 4 p m | 29 | 6 | 11 | 3 | 2 | 1 | W | 1 | fair | |
| 22 | 9 a m | 29 | 5 | 10 | 3 | 2 | 1 | S W | 1 | fair | 0,155 |
| | 4 p m | 29 | 1 | 10 | 7 | 2 | 2 | S W | 3 | cloudy | |
| 23 | 9 a m | 29 | 2 | 10 | 2 | 1 | 9 | S W | 2 | fair | 0,130 |
| | 4 p m | 29 | 3 | 10 | 6 | 1 | 8 | S W | 2 | fair | |
| 24 | 9 a m | 29 | 3 | 9 | 7 | 2 | 8 | S W | 2 | fair | 0,195 |
| | 4 p m | 29 | 5 | 9 | 7 | 2 | 0 | W | 2 | fair | |
| 25 | 9 a m | 29 | 4 | 9 | 9 | 2 | 1 | W | 2 | cloudy | 0,055 |
| | 4 p m | 29 | 4 | 9 | 5 | 2 | 1 | W by S | 2 | cloudy | |
| 26 | 9 a m | 29 | 6 | 8 | 9 | 2 | 1 | W by S | 2 | fair | 0,124 |
| | 4 p m | 29 | 7 | 9 | 4 | 2 | 0 | W by S | 2 | fair | |
| 27 | 9 a m | 29 | 2 | 9 | 0 | 2 | 2 | W | 1 | fog | |
| | 4 p m | 29 | 4 | 10 | 4 | 2 | 2 | W | 1 | fair | |
| 28 | 9 a m | 29 | 8 | 10 | 2 | 2 | 3 | S W | 0 | fog | 0,093 |
| | 4 p m | 29 | 9 | 10 | 6 | 2 | 1 | S W | 1 | cloudy | |
| 29 | 9 a m | 29 | 6 | 11 | 7 | 2 | 1 | S W | 3 | cloudy | |
| | 4 p m | 29 | 6 | 10 | 7 | 1 | 9 | S W | 2 | fair | |
| 30 | 9 a m | 29 | 5 | 11 | 0 | 2 | 0 | S W | 3 | rain | 0,075 |
| | 4 p m | 29 | 4 | 11 | 7 | 2 | 2 | S W | 3 | fair | |
| 31 | 9 a m | 29 | 2 | 11 | 3 | 2 | 2 | S W | 3 | cloudy | |
| | 4 p m | 29 | 3 | 11 | 1 | 2 | 1 | W | 2 | cloudy | |
| H. at a med. | | 29 | 5 | 10 | 7 | 2 | 1 | | | Total depth | 3,629 |
| Gr. height | | 30 | 2 | 12 | 6 | 2 | 8 | | | | |
| L. height | | 28 | 8 | 8 | 9 | 1 | 7 | | | | |

## *JANUARY* 1734.

| D | Hour. | Baro. In. | D. | Ther In. | D. | Hyg I. | D. | Wind. Dir. | For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 4 | 8 | 6 | 2 | 3 | S W | 3 | cloudy | |
| | 4 p m | 29 | 6 | 9 | 3 | 2 | 0 | W | 3 | cloudy | |
| 2 | 9 a m | 29 | 6 | 9 | 6 | 2 | 0 | S | 3 | cloudy | |
| | 4 p m | 29 | 4 | 9 | 3 | 2 | 1 | S W | 2 | rain | |
| 3 | 9 a m | 29 | 3 | 8 | 4 | 2 | 3 | W | 2 | fair | |
| | 4 p m | 29 | 3 | 8 | 5 | 2 | 1 | W | 2 | fair | |
| 4 | 9 a m | 29 | 2 | 8 | 3 | 2 | 2 | S W | 2 | fair | |
| | 4 p m | 29 | 2 | 8 | 3 | 2 | 1 | S W | 2 | fair | |
| 5 | 9 a m | 29 | 2 | 7 | 8 | 2 | 3 | S W | 2 | cloudy | |
| | 4 p m | 29 | 3 | 7 | 3 | 2 | 5 | S W | 2 | cloudy | 0,085 |
| 6 | 9 a m | 29 | 6 | 7 | 4 | 2 | 0 | W | 2 | fair | |
| | 4 p m | 29 | 6 | 7 | 7 | 1 | 9 | W | 1 | fair | |
| 7 | 9 a m | 29 | 7 | 7 | 1 | 1 | 9 | W | 2 | fair | |
| | 4 p m | 29 | 8 | 7 | 7 | 2 | 6 | W | 1 | fog | |
| 8 | 9 a m | 30 | 0 | 8 | 0 | 2 | 8 | S W | 1 | fog | |
| | 4 p m | 30 | 0 | 8 | 8 | 2 | 6 | S W | 2 | fog | |
| 9 | 9 a m | 30 | 0 | 9 | 0 | 2 | 0 | S by W | 1 | cloudy | 0,055 |
| | 4 p m | 30 | 0 | 9 | 3 | 2 | 0 | S | 1 | cloudy | |
| 10 | 9 a m | 30 | 2 | 8 | 7 | 2 | 5 | S | 1 | cloudy | |
| | 4 p m | 30 | 3 | 9 | 1 | 2 | 5 | S | 1 | cloudy | |
| 11 | 9 a m | 30 | 5 | 7 | 7 | 1 | 8 | S | 1 | fair | |
| | 4 p m | 30 | 5 | 7 | 5 | 1 | 7 | S | 1 | fair | 0,095 |
| 12 | 9 a m | 30 | 5 | 7 | 0 | 1 | 7 | S W | 1 | fair | |
| | 4 p m | 30 | 6 | 7 | 6 | 1 | 6 | S W | 1 | fair | |
| 13 | 9 a m | 30 | 5 | 6 | 2 | 1 | 5 | S W | 1 | fair | |
| | 4 p m | 30 | 4 | 7 | 4 | 1 | 7 | S W | 2 | fair | |
| 14 | 9 a m | 30 | 2 | 7 | 0 | | 0 | S W | 1 | fair | |
| | 4 p m | 30 | 1 | 7 | 4 | 1 | 2 | S W | 1 | fair | |
| 15 | 9 a m | 30 | 0 | 7 | 0 | 2 | 6 | S by E | 1 | fog | |
| | 4 p m | 29 | 9 | 7 | 3 | 2 | 5 | S by E | 1 | fog | 0,173 |
| 16 | 9 a m | 29 | 7 | 7 | 6 | 2 | 5 | S | 1 | fog | |
| | 4 p m | 29 | 7 | 8 | 2 | 2 | 3 | S | 1 | fair | |
| | | | | | | | | | | | 0,408 |

## *JANUARY* 1734.

| D | Hour. | Baro. In. | D. | Ther. In. | D | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,408 |
| 27 | 9 a m | 29 | 8 | 8 | 0 | 2 | 5 | S | 1 | fog | |
| | 4 p m | 30 | 0 | 8 | 5 | 2 | 4 | S W | 1 | cloudy | |
| 28 | 9 a m | 29 | 9 | 10 | 0 | 2 | 1 | S W | 3 | cloudy | |
| | 4 p m | 30 | 0 | 11 | 2 | 2 | 3 | S W | 1 | cloudy | |
| 19 | 9 a m | 30 | 0 | 10 | 7 | 2 | 3 | S W | 1 | cloudy | |
| | 4 p m | 30. | 1 | 10 | 5 | 2 | 1 | S W | 2 | fair | |
| 20 | 9 a m | 30 | 1 | 9 | 8 | 2 | 0 | S W | 1 | fair | |
| | 4 p m | 30 | 1 | 9 | 8 | 2 | 0 | S W | 1 | fair | |
| 21 | 9 a m | 30 | 0 | 8 | 9 | 2 | 0 | S W | 1 | fair | |
| | 4 p m | 29 | 8 | 10 | 0 | 1 | 9 | S W | 1 | fair | |
| 22 | 9 a m | 29 | 4 | 9 | 4 | 2 | 1 | S W | 2 | fair | |
| | 4 p m | 29 | 4 | 9 | 7 | 2 | 1 | W | 2 | fair | |
| 23 | 9 a m | 29 | 8 | 9 | 5 | 1 | 9 | W | 2 | fair | |
| | 4 p m | 29 | 8 | 10 | 4 | 1 | 2 | W | 2 | fair | |
| 24 | 9 a m | 29 | 8 | 11 | 1 | 2 | 5 | W | 2 | cloudy | |
| | 4 p m | 29 | 7 | 11 | 5 | 2 | 4 | W | 2 | rain | |
| 25 | 9 a m | 29 | 6 | 10 | 4 | 2 | 1 | W by S | 3 | cloudy | 0,065 |
| | 4 p m | 29 | 3 | 9 | 9 | 2 | 1 | W by S | 3 | cloudy | |
| 26 | 9 a m | 29 | 8 | 10 | 1 | 2 | 0 | N W | 2 | fair | |
| | 4 p m | 29 | 9 | 10 | 8 | 1 | 8 | W | 3 | fair | |
| 27 | 9 a m | 30 | 2 | 11 | 0 | 2 | 6 | W | 2 | fair | |
| | 4 p m | 30 | 2 | 11 | 2 | 2 | 3 | W | 3 | cloudy | |
| 28 | 9 a m | 30 | 2 | 10 | 7 | 2 | 5 | S W | 2 | fair | |
| | 4 p m | 30 | 2 | 11 | 4 | 2 | 4 | W | 2 | fair | 0,045 |
| 29 | 9 a m | 30 | 3 | 11 | 0 | 2 | 5 | W | 2 | cloudy | |
| | 4 p m | 30 | 3 | 10 | 8 | 2 | 5 | W by S | 2 | cloudy | |
| 30 | 9 a m | 30 | 3 | 11 | 2 | 2 | 5 | W | 2 | cloudy | 0,075 |
| | 4 p m | 30 | 2 | 11 | 2 | 2 | 3 | W | 2 | cloudy | |
| 31 | 9 a m | 30 | 2 | 10 | 2 | 2 | 1 | W | 2 | fair | |
| | 4 p m | 30 | 2 | 9 | 4 | 1 | 5 | N W | 3 | fair | |
| H. at a med. | | 29 | 9 | 8 | 8 | 2 | 2 | | | Total depth | 0593 |
| Gr. height | | 30 | 6 | 11 | 5 | 2 | 8 | | | | |
| L. height | | 29 | 2 | 6 | 2 | 1 | 2 | | | | |

## *FEBRUARY* 1734.

| D. | Hour. | Baro. In. | Baro. D. | Ther. In. | Ther. D. | Hyg. I. | Hyg. D. | Wind. Dir. | Wind. For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 30 | 2 | 9 | 0 | 1 | 9 | W by S | 2 | fair | |
| | 4 p m | 30 | 1 | 10 | 4 | 2 | 1 | W | 0 | cloudy | |
| 2 | 9 a m | 30 | 0 | 10 | 6 | 2 | 1 | W | 4 | fair | |
| | 4 p m | 30 | 0 | 11 | 0 | 2 | 0 | W | 3 | fair | |
| 3 | 9 a m | 29 | 9 | 10 | 8 | 2 | 3 | S W | 2 | fair | |
| | 4 p m | 29 | 7 | 11 | 2 | 2 | 2 | W | 3 | fair | |
| 4 | 9 a m | 29 | 9 | 9 | 9 | 1 | 9 | W | 3 | fair | 0,114 |
| | 5 p m | 29 | 8 | 10 | 3 | 2 | 0 | W | 3 | cloudy | |
| 5 | 9 a m | 29 | 8 | 10 | 8 | 2 | 3 | W | 2 | cloudy | |
| | 4 p m | 29 | 9 | 11 | 4 | 2 | 1 | W | 2 | cloudy | |
| 6 | 9 a m | 29 | 8 | 10 | 9 | 2 | 2 | S W | 2 | cloudy | |
| | 4 p m | 29 | 6 | 11 | 0 | 2 | 0 | S W | 3 | cloudy | |
| 7 | 9 a m | 29 | 6 | 10 | 3 | 2 | 0 | W by N | 3 | fair | 0,094 |
| | 4 p m | 29 | 8 | 10 | 4 | 1 | 4 | N W | 3 | fair | |
| 8 | 9 a m | 30 | 3 | 9 | 9 | 1 | 6 | W | 1 | cloudy | |
| | 4 p m | 30 | 3 | 10 | 5 | 2 | 0 | W | 2 | cloudy | |
| 9 | 9 a m | 30 | 2 | 11 | 0 | 2 | 3 | S by W | 3 | cloudy | |
| | 4 p m | 30 | 0 | 11 | 3 | 2 | 0 | S W | 2 | cloudy | |
| 10 | 9 a m | 29 | 9 | 10 | 4 | 2 | 0 | N W | 3 | fair | 0,243 |
| | 4 p m | 30 | 0 | 10 | 6 | 1 | 8 | N W | 3 | fair | |
| 11 | 9 a m | 30 | 0 | 10 | 7 | 1 | 9 | S W | 2 | cloudy | |
| | 4 p m | 29 | 9 | 11 | 1 | 1 | 8 | S W | 2 | cloudy | |
| 12 | 9 a m | 29 | 6 | 11 | 7 | 2 | 0 | S W | 2 | cloudy | |
| | 4 p m | 29 | 6 | 11 | 8 | 1 | 7 | S W | 1 | cloudy | |
| 13 | 9 a m | 29 | 4 | 11 | 8 | 2 | 0 | S W | 2 | fair | 0,023 |
| | 4 p m | 29 | 5 | 11 | 5 | 1 | 6 | S W | 2 | fair | |
| 14 | 9 a m | 29 | 6 | 9 | 8 | 1 | 7 | S W | 2 | fair | |
| | 4 p m | 29 | 5 | 9 | 6 | 1 | 5 | S W | 3 | cloudy | |
| 15 | 9 a m | 28 | 8 | 10 | 5 | 2 | 0 | S by W | 4 | rain | |
| | 4 p m | 28 | 6 | 10 | 2 | 2 | 0 | S W | 4 | cloudy | |
| 16 | 9 a m | 29 | 0 | 9 | 2 | 2 | 0 | S W | 3 | fair | |
| | 4 p m | 29 | 0 | 9 | 4 | 2 | 0 | W | 2 | cloudy | |
| | | | | | | | | | | | 0,474 |

## *FEBRUARY* 1734.

| D. | Hour | Baro. In. | D. | Ther. In. | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,474 |
| 17 | 9 a m | 29 | 0 | 9 | 2 | 1 | 8 | S W | 3 | cloudy | |
| | 5 p m | 29 | 1 | 8 | 6 | 2 | 2 | S W | 3 | cloudy | |
| 18 | 9 a m | 29 | 0 | 9 | 9 | 2 | 1 | N W | 3 | cloudy | |
| | 5 p m | 29 | 5 | 10 | 4 | 1 | 5 | N | 4 | fair | |
| 19 | 9 a m | 29 | 7 | 9 | 1 | 1 | 7 | N W | 2 | fair | 0,065 |
| | 4 p m | 29 | 5 | 10 | 2 | 1 | 7 | W | 2 | rain | |
| 20 | 9 a m | 29 | 5 | 9 | 1 | 2 | 0 | N W | 3 | cloudy | |
| | 5 p m | 29 | 7 | 9 | 7 | 1 | 5 | N W | 2 | fair | |
| 21 | 9 a m | 29 | 9 | 9 | 1 | 1 | 9 | W | 3 | cloudy | |
| | 4 p m | 29 | 9 | 9 | 6 | 2 | 0 | W | 2 | rain | |
| 22 | 9 a m | 29 | 7 | 10 | 9 | 2 | 6 | S W | 2 | fair | |
| | 5 p m | 29 | 5 | 11 | 6 | 2 | 1 | S W | 2 | cloudy | |
| 23 | 9 a m | 29 | 3 | 10 | 9 | 2 | 1 | S W | 2 | cloudy | |
| | 5 p m | 29 | 3 | 11 | 5 | 1 | 7 | S W | 2 | cloudy | |
| 24 | 9 a m | 28 | 9 | 10 | 6 | 2 | 0 | S W | 2 | cloudy | |
| | 5 p m | 28 | 9 | 10 | 6 | 1 | 7 | W | 1 | fair | |
| 25 | 9 a m | 29 | 0 | 10 | 1 | 2 | 2 | S | 1 | cloudy | |
| | 5 p m | 29 | 1 | 10 | 8 | 2 | 1 | S | 1 | cloudy | |
| 26 | 9 a m | 29 | 3 | 9 | 6 | 2 | 1 | S | 1 | fair | 0,056 |
| | 4 p m | 29 | 3 | 10 | 8 | 2 | 0 | S | 1 | fair | |
| 27 | 9 a m | 29 | 6 | 9 | 7 | 2 | 0 | S W | 0 | fair | |
| | 4 p m | 29 | 6 | 11 | 4 | 1 | 6 | S W | 2 | cloudy | |
| 28 | 9 a m | 29 | 4 | 11 | 4 | 1 | 8 | S | 4 | cloudy | |
| | 5 p m | 29 | 5 | 12 | 2 | 1 | 5 | S W | 3 | fair | |
| H. at a med. | | 29 | 6 | 10 | 5 | 1 | 9 | | | Total depth | 0,595 |
| Gr. height | | 30 | 3 | 12 | 2 | 2 | 6 | | | | |
| L. height | | 28 | 6 | 8 | 6 | 1 | 4 | | | | |

## *MARCH* 1734.

| D. | Hour | Baro. | | Ther. | | Hyg. | | Wind. | | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | In | D | In | D. | I. | D. | Dir. | For. | | In D. |
| 1 | 9 a m | 29 | 7 | 10 | 7 | 1 | 8 | S | 1 | fair | |
| | 5 p m | 29 | 7 | 11 | 7 | 1 | 6 | S E | 2 | fair | |
| 2 | 9 a m | 29 | 6 | 10 | 7 | 1 | 7 | S by E | 1 | cloudy | |
| | 5 p m | 29 | 6 | 11 | 4 | 2 | 0 | S E | 1 | fair | |
| 3 | 9 a m | 29 | 6 | 11 | 5 | 2 | 1 | S W | 1 | cloudy | |
| | 5 p m | 29 | 6 | 11 | 9 | 1 | 8 | S W | 1 | fair. | |
| 4 | 9 a m | 29 | 5 | 12 | 1 | 1 | 9 | S | 2 | cloudy | |
| | 6 p m | 29 | 4 | 11 | 5 | 1 | 8 | S | 1 | cloudy | |
| 5 | 9 a m | 29 | 5 | 11 | 2 | 1 | 8 | S W | 1 | rain | |
| | 5 p m | 29 | 4 | 11 | 6 | 1 | 6 | S W | 1 | cloudy | |
| 6 | 9 a m | 29 | 1 | 12 | 3 | 2 | 0 | S W | 3 | cloudy | 0,195 |
| | 4 p m | 29 | 2 | 12 | 3 | 1 | 7 | S W | 3 | fair | |
| 7 | 9 a m | 29 | 2 | 11 | 6 | 1 | 7 | W | 3 | fair | |
| | 5 p m | 29 | 4 | 11 | 3 | 1 | 7 | W by N | 3 | fair | |
| 8 | 9 a m | 29 | 7 | 9 | 6 | 1 | 8 | W by N | 2 | fair | 0,055 |
| | 5 p m | 29 | 7 | 10 | 5 | 1 | 6 | W by N | 2 | cloudy | |
| 9 | 9 a m | 29 | 6 | 11 | 6 | 1 | 6 | W | 2 | cloudy | 0,025 |
| | 5 p m | 29 | 6 | 10 | 8 | 1 | 6 | W | 2 | cloudy | |
| 10 | 9 a m | 29 | 7 | 9 | 9 | 2 | 0 | W by N | 2 | fair | 0,048 |
| | 5 p m | 29 | 8 | 10 | 7 | 1 | 5 | N W | 2 | fair | |
| 11 | 9 a m | 29 | 8 | 9 | 9 | 1 | 8 | S E | 2 | fair | 0,036 |
| | 6 p m | 29 | 6 | 11 | 1 | 1 | 9 | S | 2 | cloudy | |
| 12 | 9 a m | 29 | 6 | 10 | 8 | 1 | 8 | S W | 3 | rain | 0,095 |
| | 5 p m | 29 | 7 | 10 | 7 | 1 | 6 | W | 3 | fair | |
| 13 | 9 a m | 29 | 5 | 9 | 8 | 1 | 7 | S E | 2 | fog | |
| | 5 p m | 29 | 3 | 10 | 6 | 1 | 9 | S E | 1 | rain | |
| 14 | 9 a m | 29 | 6 | 9 | 5 | 2 | 0 | S by E | 0 | fair | 0,172 |
| | 5 p m | 29 | 5 | 11 | 5 | 1 | 7 | S E | 0 | cloudy | |
| 15 | 9 a m | 29 | 4 | 10 | 4 | 1 | 0 | W | 3 | cloudy | 0,055 |
| | 5 p m | 29 | 6 | 11 | 3 | 1 | 4 | W by N | 2 | fair | |
| 16 | 9 a m | 29 | 7 | 11 | 3 | 1 | 8 | S by W | 2 | cloudy | 0,026 |
| | 6 p m | 29 | 5 | 11 | 5 | 1 | 6 | S W | 2 | cloudy | |
| | | | | | | | | | | | 0,707 |

## *MARCH* 1734.

| D. | Hour. | Baro. In D. | | Ther. In D. | | Hyg I. D. | | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,707 |
| 17 | 9 a m | 29 | 8 | 9 | 6 | 1 | 8 | S W | 3 | fair | 0,034 |
| | 5 p m | 29 | 8 | 10 | 8 | 1 | 3 | W by S | 4 | cloudy | |
| 18 | 9 a m | 29 | 5 | 10 | 6 | 1 | 9 | S by W | 1 | cloudy | 0,195 |
| | 6 p m | 29 | 4 | 11 | 3 | 1 | 9 | S by W | 1 | cloudy | |
| 19 | 9 a m | 29 | 4 | 10 | 8 | 1 | 9 | S by E | 0 | fair | 0,075 |
| | 5 p m | 29 | 4 | 11 | 0 | 1 | 4 | S W | 1 | fair | |
| 20 | 9 a m | 29 | 2 | 11 | 2 | 1 | 8 | S E | 1 | rain | 0,113 |
| | 6 p m | 29 | 1 | 12 | 5 | 1 | 9 | S by W | 1 | cloudy | |
| 21 | 9 a m | 29 | 1 | 11 | 5 | 1 | 8 | S W | 1 | fair | 0,093 |
| | 5 p m | 29 | 4 | 11 | 0 | 1 | 7 | W | 2 | fair | |
| 22 | 9 a m | 29 | 5 | 11 | 2 | 2 | 0 | S W | 2 | fair | 0,204 |
| | 5 m | 29 | 5 | 12 | 5 | 1 | 4 | S W | 3 | cloudy | |
| 23 | 9 a m | 29 | 4 | 11 | 8 | 1 | 7 | S W | 4 | variable | 0,265 |
| | 6 p m | 29 | 4 | 10 | 7 | 1 | 5 | S W | 4 | rain | |
| 24 | 9 a m | 29 | 6 | 10 | 8 | 1 | 7 | S W | 3 | fair | |
| | 5 p m | 29 | 6 | 11 | 4 | 1 | 4 | S W | 2 | cloudy | |
| 25 | 9 a m | 29 | 6 | 10 | 6 | 1 | 7 | S W | 2 | fair | |
| | 5 p m | 29 | 6 | 1 | 5 | 1 | 3 | W by S | 2 | fair | |
| 26 | 9 a m | 29 | 3 | 11 | 3 | 1 | 7 | S W | 2 | cloudy | 0,210 |
| | 4 p m | 29 | 1 | 11 | 4 | 1 | 8 | S W | 1 | rain | |
| 27 | 9 a m | 29 | 4 | 11 | 3 | 1 | 6 | N W | 2 | fair | 0,034 |
| | 6 p m | 29 | 5 | 11 | 4 | 1 | 4 | N W | 2 | fair | |
| 28 | 9 a m | 29 | 7 | 9 | 6 | 1 | 6 | N W | 1 | fair | |
| | 6 p m | 29 | 6 | 11 | 0 | 1 | 5 | W | 1 | cloudy | |
| 29 | 9 a m | 29 | 5 | 11 | 2 | 1 | 5 | W by N | 3 | fair | 0,044 |
| | 5 p m | 29 | 5 | 11 | 4 | 1 | 4 | W by N | 3 | fair | |
| 30 | 9 a m | 29 | 7 | 11 | 2 | 1 | 5 | W by N | 2 | fair | 0,085 |
| | 5 p m | 29 | 7 | 12 | 3 | 1 | 7 | S W | 3 | cloudy | |
| 31 | 9 a m | 29 | 8 | 12 | 4 | 2 | 1 | W by N | 3 | cloudy | 0,063 |
| | 5 p m | 29 | 9 | 12 | 4 | 1 | 7 | W by N | 2 | fair | |
| H at a med. | | 29 | 5 | 11 | 1 | 1 | 7 | | | Total depth | 2,12[illegible] |
| Gr. height | | 29 | 9 | 12 | 5 | 2 | 1 | | | | |
| L. height | | 29 | 1 | 9 | 5 | 1 | 3 | | | | |

## APRIL 1734.

| D. | Hour. | Baro. In. | D. | Ther. In. | D. | Hyg. I. | D. | Wind. Dir. | For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 9 | 12 | 3 | 2 | 0 | S W | 2 | cloudy | |
| | 6 p m | 29 | 9 | 12 | 8 | 1 | 7 | S W | 3 | cloudy | |
| 2 | 9 a m | 29 | 9 | 12 | 2 | 1 | 9 | S W | 3 | fair | 0,125 |
| | 4 p m | 30 | 0 | 13 | 2 | 1 | 8 | W | 2 | cloudy | |
| 3 | 9 a m | 30 | 0 | 12 | 4 | 1 | 9 | W | 2 | cloudy | |
| | 5 p m | 30 | 0 | 12 | 6 | 1 | 7 | W | 2 | cloudy | |
| 4 | 9 a m | 29 | 9 | 12 | 3 | 2 | 0 | W | 2 | fair | |
| | 4 p m | 29 | 9 | 13 | 7 | 1 | 5 | W | 2 | fair | |
| 5 | 9 a m | 30 | 0 | 11 | 3 | 1 | 7 | W | 2 | fair | 0,034 |
| | 5 p m | 30 | 0 | 12 | 4 | 1 | 3 | N E | 2 | fair | |
| 6 | 9 a m | 29 | 8 | 13 | 3 | 1 | 5 | S E | 0 | cloudy | |
| | 5 p m | 29 | 6 | 13 | 1 | 1 | 6 | S W | 2 | cloudy | |
| 7 | 9 a m | 29 | 6 | 10 | 9 | 1 | 5 | W | 2 | fair | |
| | 6 p m | 29 | 6 | 11 | 1 | 1 | 7 | W | 3 | fair | |
| 8 | 9 a m | 29 | 6 | 11 | 5 | 1 | 8 | S W | 2 | cloudy | |
| | 4 p m | 29 | 5 | 11 | 7 | 1 | 5 | W by N | 2 | fair | |
| 9 | 9 a m | 29 | 5 | 10 | 5 | 1 | 6 | W by N | 3 | fair | |
| | 4 p m | 29 | 6 | 11 | 9 | 1 | 2 | N W | 3 | fair | |
| 10 | 9 a m | 29 | 8 | 12 | 1 | 2 | 0 | W by S | 3 | cloudy | |
| | 5 p m | 29 | 8 | 12 | 4 | 1 | 5 | W | 3 | cloudy | |
| 11 | 9 a m | 29 | 9 | 11 | 4 | 1 | 6 | W | 2 | fair | |
| | 6 p m | 30 | 0 | 11 | 6 | 1 | 4 | W | 4 | variable | |
| 12 | 9 a m | 30 | 0 | 11 | 8 | 1 | 5 | W by S | 2 | cloudy | |
| | 5 p m | 30 | 0 | 12 | 7 | 1 | 5 | S W | 3 | cloudy | 0,055 |
| 13 | 9 a m | 30 | 1 | 13 | 2 | 1 | 9 | W | 2 | fair | |
| | 5 p m | 30 | 2 | 13 | 7 | 1 | 5 | W by S | 2 | fair | |
| 14 | 9 a m | 30 | 2 | 12 | 7 | 1 | 9 | W by S | 1 | fair | |
| | 5 p m | 30 | 1 | 14 | 7 | 1 | 4 | S by E | 1 | fair | |
| 15 | 9 a m | 30 | 0 | 12 | 5 | 1 | 8 | S E | 1 | fair | |
| | 5 p m | 29 | 9 | 14 | 0 | 1 | 3 | S E | 1 | fair | |
| 16 | 9 a m | 29 | 9 | 12 | 3 | 1 | 8 | S E | 1 | fair | |
| | 5 p m | 29 | 9 | 13 | 4 | 1 | 3 | E | 0 | fair | |
| | | | | | | | | | | | 0,214 |

## *APRIL* 1734.

| D | Hour | Baro. In | D | Ther. In. | D | Hyg. I. | D | Wind. Dir. | For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,214 |
| 17 | 9 a m | 29 | 9 | 11 | 7 | 1 | 9 | E by N | 2 | fair | |
| | 5 p m | 29 | 9 | 11 | 8 | 1 | 6 | N E | 2 | fair | |
| 18 | 9 a m | 29 | 7 | 10 | 7 | 2 | 4 | E | 3 | cloudy | 0,137 |
| | 4 p m | 29 | 8 | 11 | 5 | 1 | 8 | E | 2 | cloudy | |
| 19 | 9 a m | 30 | 0 | 11 | 5 | 1 | 9 | E | 2 | fair | 0,204 |
| | 6 p m | 30 | 0 | 11 | 2 | 1 | 8 | E | 2 | cloudy | |
| 20 | 9 a m | 30 | 0 | 11 | 5 | 2 | 6 | E | 2 | cloudy | 0,055 |
| | 7 p m | 30 | 0 | 11 | 1 | 2 | 0 | E | 2 | fair | |
| 21 | 9 a m | 30 | 0 | 11 | 5 | 1 | 6 | E | 1 | fair | |
| | 6 p m | 29 | 9 | 12 | 6 | 1 | 4 | E | 1 | fair | |
| 22 | 9 a m | 29 | 9 | 9 | 9 | 1 | 8 | N E | 2 | cloudy | 0,045 |
| | 6 p m | 29 | 9 | 9 | 4 | 2 | 1 | E | 1 | rain | |
| 23 | 9 a m | 29 | 8 | 11 | 3 | 2 | 0 | E | 1 | cloudy | |
| | 6 p m | 29 | 8 | 11 | 4 | 1 | 8 | E | 2 | fair | |
| 24 | 9 a m | 29 | 7 | 11 | 4 | 2 | 0 | S E | 2 | cloudy | |
| | 6 p m | 29 | 6 | 11 | 6 | 2 | 1 | S E | 2 | cloudy | |
| 25 | 9 a m | 29 | 5 | 12 | 7 | 2 | 3 | S E | 1 | fair | |
| | 6 p m | 29 | 6 | 13 | 0 | 1 | 6 | W | 1 | cloudy | |
| 26 | 9 a m | 29 | 5 | 13 | 5 | 1 | 6 | S W | 2 | fair | 0,056 |
| | 6 p m | 29 | 5 | 13 | 1 | 1 | 4 | W | 2 | fair | |
| 27 | 9 a m | 29 | 5 | 12 | 6 | 1 | 6 | S W | 2 | fair | 0,084 |
| | 5 p m | 29 | 5 | 13 | 0 | 1 | 6 | W | 1 | cloudy | |
| 28 | 9 a m | 29 | 5 | 12 | 4 | 1 | 6 | W | 2 | fair | 0,095 |
| | 6 p m | 29 | 5 | 13 | 5 | 1 | 3 | S W | 2 | fair | |
| 29 | 9 a m | 29 | 5 | 13 | 5 | 1 | 3 | S W | 2 | fair | 0,116 |
| | 6 p m | 29 | 4 | 13 | 6 | 1 | 4 | S W | 1 | rain | |
| 30 | 9 a m | 29 | 4 | 12 | 6 | 1 | 4 | S W | 2 | fair | |
| | 5 p m | 29 | 4 | 12 | 4 | 1 | 4 | S W | 2 | cloudy | |
| H. at a med. | | 29 | 8 | 12 | 2 | 1 | 7 | | | Total depth | 1,006 |
| Gr. height | | 30 | 2 | 14 | 7 | 2 | 6 | | | | |
| L. height | | 29 | 4 | 9 | 4 | 1 | 2 | | | | |

## *MAY* 1734.

| D. | Hour. | Baro In D. | | Ther. In D. | | Hyg. I. D. | | Wind. Dir. | For. | Weather. | Rain. In D. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 9 a m | 29 | 4 | 12 | 2 | 1 | 3 | S W | 2 | cloudy | |
| | 5 p m | 29 | 4 | 12 | 0 | 1 | 5 | S W | 2 | rain | |
| 2 | 7 a m | 29 | 7 | 9 | 8 | 1 | 7 | N E | 2 | fair | |
| | 5 p m | 29 | 7 | 10 | 8 | 1 | 4 | N E | 2 | fair | 0,866 |
| 3 | 9 a m | 29 | 8 | 10 | 4 | 1 | 7 | N | 2 | cloudy | |
| | 6 p m | 29 | 8 | 11 | 6 | 1 | 2 | N by W | 2 | fair | |
| 4 | 9 a m | 29 | 8 | 11 | 0 | 1 | 4 | N W | 2 | fair | |
| | 5 p m | 29 | 9 | 12 | 3 | 1 | 0 | N W | 2 | fair | |
| 5 | 9 a m | 30 | 0 | 11 | 2 | 1 | 2 | W | 1 | fair | 0,057 |
| | 5 p m | 29 | 9 | 12 | 6 | 1 | 6 | W | 2 | cloudy | |
| 6 | 9 a m | 29 | 7 | 11 | 9 | 1 | 5 | W | 2 | cloudy | |
| | 7 p m | 29 | 8 | 11 | 2 | 1 | 5 | W | 1 | cloudy | |
| 7 | 9 a m | 29 | 7 | 11 | 4 | 1 | 5 | W | 1 | cloudy | |
| | 5 p m | 29 | 6 | 10 | 8 | 1 | 7 | N E | 1 | cloudy | |
| 8 | 9 a m | 29 | 7 | 12 | 0 | 1 | 6 | E | 2 | fair | 0,127 |
| | 5 p m | 29 | 8 | 13 | 1 | 1 | 3 | E | 2 | fair | |
| 9 | 9 a m | 30 | 0 | 11 | 8 | 1 | 4 | N E | 2 | fair | 0,055 |
| | 5 p m | 30 | 1 | 12 | 7 | 1 | 6 | N E | 2 | fair | |
| 10 | 9 a m | 30 | 0 | 12 | 0 | 1 | 5 | W by N | 2 | cloudy | |
| | 5 p m | 29 | 9 | 12 | 8 | 1 | 3 | N W | 3 | fair | |
| 11 | 9 a m | 29 | 7 | 12 | 2 | 1 | 5 | W | 4 | cloudy | |
| | 7 p m | 29 | 4 | 12 | 4 | 1 | 5 | W | 3 | cloudy | |
| 12 | 9 a m | 29 | 4 | 11 | 5 | 1 | 4 | N W | 3 | fair | |
| | 5 p m | 29 | 3 | 12 | 6 | 1 | 0 | N W | 3 | cloudy | |
| 13 | 9 a m | 29 | 4 | 11 | 0 | 1 | 3 | N W | 3 | fair | |
| | 5 p m | 29 | 4 | 11 | 9 | 1 | 2 | S | 1 | hail | |
| 14 | 9 a m | 29 | 4 | 11 | 0 | 1 | 4 | E | 2 | fair | 0,185 |
| | 5 p m | 29 | 4 | 11 | 3 | 1 | 3 | E | 2 | fair | |
| 15 | 9 a m | 29 | 5 | 10 | 6 | 1 | 4 | N W | 1 | fair | 0,205 |
| | 7 p m | 29 | 5 | 11 | 7 | 1 | 3 | N | 1 | cloudy | |
| 16 | 9 a m | 29 | 6 | 11 | 9 | 1 | 5 | E by S | 1 | cloudy | 0,113 |
| | 7 p m | 29 | 6 | 11 | 9 | 1 | 5 | E | 1 | rain | |
| | | | | | | | | | | | 0,808 |

## *MAY* 1735.

| D. | Hour. | Baro. In | Baro. D. | Ther. In | Ther. D. | Hyg I. | Hyg D. | Wind. Dir. | Wind. For. | Weather. | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | 0,808 |
| 17 | 9 a m | 29 | 6 | 11 | 2 | 1 | 3 | N W | 1 | fair | 0,130 |
| | 6 p m | 29 | 6 | 12 | 6 | 1 | 2 | E | 2 | fair | |
| 18 | 9 a m | 29 | 5 | 12 | 6 | 1 | 4 | N E | 1 | fair | |
| | 6 p m | 29 | 5 | 12 | 2 | 1 | 4 | S W | 2 | fair | |
| 19 | 9 a m | 29 | 5 | 11 | 8 | 1 | 3 | S W | 2 | fair | 0,145 |
| | 6 p m | 29 | 6 | 11 | 9 | 1 | 2 | S W | 1 | fair | |
| 20 | 9 a m | 29 | 8 | 12 | 3 | 1 | 7 | W | 2 | cloudy | 0,070 |
| | 6 p m | 29 | 8 | 13 | 0 | 1 | 4 | W | 2 | cloudy | |
| 21 | 9 a m | 29 | 8 | 13 | 9 | 1 | 4 | S | 1 | fair | 0,110 |
| | 6 p m | 29 | 6 | 12 | 6 | 1 | 7 | S W | 2 | cloudy | |
| 22 | 9 a m | 29 | 4 | 13 | 1 | 1 | 7 | S W | 1 | cloudy | 0,398 |
| | 7 p m | 29 | 4 | 13 | 1 | 1 | 4 | S W | 1 | cloudy | |
| 23 | 9 a m | 29 | 6 | 12 | 2 | 1 | 5 | W | 2 | fair | 0,127 |
| | 7 p m | 29 | 7 | 12 | 5 | 1 | 3 | W | 2 | fair | |
| 24 | 9 a m | 29 | 8 | 13 | [illegible] | 1 | 5 | W | 1 | cloudy | 0,088 |
| | 6 p m | 29 | 8 | 13 | 7 | 1 | 3 | E | 1 | fair | |
| 25 | 9 a m | 29 | 8 | 12 | 8 | 1 | 5 | E | 2 | fair | 0,034 |
| | 8 p m | 29 | 7 | 11 | 9 | 1 | 6 | E | 2 | cloudy | |
| 26 | 9 a m | 29 | 6 | 11 | 7 | 2 | 0 | N E | 2 | cloudy | |
| | 7 p m | 29 | 7 | 11 | 7 | 2 | 1 | N E | 2 | cloudy | |
| 27 | 9 a m | 29 | 7 | 12 | 0 | 1 | 8 | N E | 1 | fair | |
| | 6 p m | 29 | 7 | 13 | 0 | 1 | 6 | E | 1 | fair | |
| 28 | 9 a m | 29 | 7 | 13 | 6 | 2 | 0 | N E | 1 | fair | 0.155 |
| | 7 p m | 29 | 7 | 13 | 3 | 1 | 3 | S E | 1 | cloudy | |
| 29 | 9 a m | 29 | 6 | 12 | 7 | 1 | 2 | E | 1 | cloudy | 0,085 |
| | 6 p m | 29 | 6 | 12 | [illegible] | 2 | 2 | E | 1 | cloudy | |
| 30 | 9 a m | 29 | 5 | 12 | 2 | 3 | 5 | E by N | 2 | cloudy | 0,840 |
| | 5 p m | 29 | 5 | 13 | 0 | 2 | 6 | E by N | 2 | cloudy | |
| 31 | 9 a m | 29 | 5 | 12 | 3 | 2 | 4 | N E | 3 | rain | 0,213 |
| | 5 p m | 29 | 6 | 11 | 8 | 3 | 3 | N E | 3 | rain | |
| H. at a med. | | 29 | 8 | 12 | 1 | 1 | 5 | | | Total depth | 3,313 |
| Gr. height | | 30 | 1 | 13 | 9 | 3 | 5 | | | | |
| L. height | | 29 | 3 | 9 | 8 | 1 | 0 | | | | |

## II. *An Account of the DISEASES that were most frequent last Year in* Edinburgh.

IN June 1733, ſeveral inhabitants of Edinburgh were ſeized with tertian agues, and others laboured under ſlight fevers, with pain of the head, and flying pains through the body, the ſick having ſhort remiſſions of the fever, and partial ſweatings. At the ſame time ſcarlet fevers and ſore throats were frequent in ſeveral parts of the country near the city.

In July many complained of anginæ, coughs, hoarſeneſs, and other effects of what they call ſummer-colds, and children were attacked with the ſcarlet-fever and angina, which became very epidemic in the two ſucceeding months, was leſs frequent and milder in October; but continued all the winter and ſpring. This diſeaſe began commonly with a quick pulſe, heat, thirſt, head-ach, and a pain in the throat, where frequently a ſwelling of the amygdalae was obſerved. Many had a vomiting and diarrhoea, at the firſt attack of this diſeaſe, without any remarkable change on the other ſymptoms. After a day or two, the face, or extremities, and ſometimes the whole body ſwelled, the ſkin being red, with a watery clearneſs ſhining through it. Frequently the ſwelling and redneſs proceeded gradually from one part to another. It was remarked, that ſuch patients who had undergone the ſcarlet-fever any time of their lives before, took at this time the fever and angina, without the ſcarlet eruption,

eruption, but all who laboured under the ſcarlet-fever had the angina alſo.

Many who were neglected in the beginning of this diſeaſe, were ſuffocated by the anginæ. Few died who were timely and plentifully blooded, which weakened the fever, relieved the throat, and was the only medicine that removed the vomiting and diarrhœa. After the pulſe was brought down with the loſs of blood, veſicatories were of uſe, and the cure was afterwards haſtened and completed by purging the patients with aperient laxative ptizans.

In the account of epidemical diſeaſes in vol. 2. we mentioned the ſmall-pox having attacked ſome children through all the ſpring of 1733, the numbers increaſing in May, which they continued to do in June, July, and Auguſt, but were then generally of the diſtinct mild kind. As they became more frequent in September, there were more of them confluent, and the number of patients increaſed all October, after which they raged moſt violently, few families eſcaping them till February, when they decreaſed, and gradually went off in March. The pocky pimples generally began to appear the third day from the ſickning, and yet were not always either confluent or dangerous. Very few had any purple ſpots with the ſmall-pox; and, of thoſe who recovered of the confluent kind, fewer had tumors or ulcers than at other times.

The cool regimen was generally followed; and, when the fever was high in the beginning, with the head or breathing much affected, the

patients were commonly blooded and vomited; and, in the confluent kind, it was neceſſary and proved uſeful to repeat the blooding about the height of the diſeaſe, and ſome time after. From the firſt attack of the fever, till the ſmall-pox were all out, many ordered pediluvia of warm water once or twice a-day, which ſeemed in many caſes to relieve the head conſiderably, and to bring a greater number of pimples to the lower extremities. When the patients were coſtive, diluent cooling clyſters were injected. Syrup of white poppies was generally given at night to procure ſleep. In ſome who had bloody ſtools and urine, the ſpirit of vitriol, mixed in pretty large quantities with the drink of the ſick, did very remarkable ſervice, theſe threatning ſymptoms diſappearing in a little time after they began to take ſuch acidulated liquors. Veſicatories aſſiſted the eruption when the pulſe was low, and often were of ſervice in removing raving, ſtartings of the nerves, convulſions, and difficult laborious breathing; aud ſome who had a bad confluent ſmall-pox, ſeemed to have the dangerous ſymptoms at the blackening prevented, by applying veſicatories a day before the blackening began, and by keeping up a ſuppuration in the bliſtered parts for ſome days. Gentle emetics were alſo advantageouſly given, when the ſtomach or lungs appeared to be overcharged with mucus. When the ſmall-pox were empty, or had only a little watery matter in them, and the ſwellings began to fall ſuddenly on the 9th or 10th day, purgatives were given to ſome with good ſucceſs.

Though

Though blooding in the beginning of the ſmall-pox evidently gave relief in a great many caſes, yet it could not well be judged, whether the taking blood before the variolous fever began, or after the ſymptoms appeared, had any effect in determining the nature or number of the ſmall-pox; for many, who had been prepared by blooding, purging, iſſues, and low cooling diet, had a very bad confluent ſmall-pox; and others who had alſo been treated in that manner, and a great number, who had uſed no ſuch precautions, took the mild kind. Some who had undergone courſes of mercury, and who had been afterwards kept for a conſiderable time to the conſtant uſe of Æthiops mineral, were ſeized with the confluent ſmall-pox and died.

During the harveſt months of 1733, dyſenteries were frequent and mortal in Fife, eſpecially on the coaſt of the Frith of Forth.

In March and April 1734, tertian agues became frequent in Edinburgh, and were very irregular; but repeated vomits either carried them off, or made them eaſily yield to the bark. At the ſame time many children in the neighbouring villages were ſeized with a very ſharp fever, and high laborious breathing, which ſoon killed them, unleſs they were timely relieved by frequent and plentiful bleeding, and gentle vomits.

III. *An Extract from the public Register of Burials in* Edinburgh.

| 1733. | Men. | Women. | Child. | Still-born. | Sum. |
|---|---|---|---|---|---|
| June | 19 | 29 | 40 | 4 | 92 |
| July | 17 | 19 | 41 | 2 | 79 |
| August | 20 | 26 | 63 | 4 | 113 |
| September | 13 | 21 | 65 | 3 | 102 |
| October | 15 | 26 | 106 | 6 | 153 |
| November | 19 | 27 | 144 | 8 | 198 |
| December | 21 | 27 | 116 | 4 | 168 |
| 1734. | | | | | |
| January | 26 | 46 | 80 | 3 | 155 |
| February | 22 | 23 | 57 | 4 | 106 |
| March | 28 | 27 | 66 | 4 | 125 |
| April | 25 | 31 | 50 | 3 | 109 |
| May | 31 | 46 | 41 | 9 | 127 |
| Total | 256 | 348 | 869 | 54 | 1527 |

IV. *The Effects of the Conessi Bark, by*——.

Gentlemen,

"NOT having allowance from my friend, who wrote me the inclosed letter, to publish his name, I cannot take upon me to communicate it; but can assure you, his integrity, good sense, and knowledge, are such as may give entire credit to his information; and lately I cured an obstinate dysentery of three months standing, which had yielded nothing to a great variety of other medicines, by giving the Conessi bark in the form prescribed by him. I am

Your most humble servant,

ALEX. MONRO."

THE

THE tree, of which I gave you ſome of the bark as a ſpecific in diarrhoeas, grows on the Cormandel coaſt in the Eaſt-Indies, where it is called Coneſſi, and is not unlike the Cadogapala of the *Hortus Malabaricus*. The coneſſiſeca, or coneſſi-bark of the ſmall young branches of the tree which has leaſt moſs, or external inſipid ſcurf on it, is to be choſen, and all that ſcurf is to be ſcraped off.

The clean bark being pounded into a very fine powder, is made into an electuary, with ſyrup of oranges, and taken to the quantity of half a drachm, or more, four times a-day, for three or four days. The firſt day it increaſes the number and quantity of the ſtools, but without increaſing the gripes. The ſecond day the bark is taken, the colour of the ſtools is mended; and on the third or fourth day, their conſiſtence generally comes near to a natural ſtate, when it ſucceeds at all.

In recent diarrhœæ, ariſing from irregularities in diet, without a fever, this medicine ſeldom fails to make a cure, if a vomit of ipecacuanna is given immediately before the patient begins the uſe of the bark. The ſame management alſo is attended commonly with ſucceſs in perſons of a lax habit of body, who are troubled with an habitual diarrhoea in moiſt rainy weather, a remarkable itching in the ſkin being felt on the third or fourth day. To ſuch patients eſpecially, the electuary ought to be given morning and evening, for ſome time after they are ſeemingly cured. Their drink ſhould be water wherein rice hath been boiled, and

and ſometimes emulſions of the cold ſeeds, with *ſal prunell.* are neceſſary.

If there is a fever with the looſeneſs, that muſt be removed by bloodings and cool emulſions, or *decoct. alb.* with *ſal prunell.* before the coneſſi bark is given.

Sometimes when the cauſe of a diarrhoea, ſtopped by this medicine, lies beyond the inteſtinal canal, the patient, in a few days after, complains of a pain in the right hypochonder, or in the right ſhoulder, or over the ſtomach towards the left ſide, cauſing often a dull ſenſe of pain, near or above the left clavicle, with a feveriſh pulſe. As ſoon as theſe ſymptoms appear, the patient muſt be blooded, and his blood will be ſizy, or with a tough yellowiſh cruſt on the top, when it has coagulated. The quantity of blood to be taken away, and the repeating the venæſection, muſt be determined by the patient's ſtrength, the degree of fever, and ſharpneſs of his pain. In ſuch caſes however the blooding ſeldom removes the pain entirely; but, after the fever is brought ſufficiently down by the loſs of blood, I have ſeldom miſſed to complete the cure, by giving ſweet mercury, or rather calomel for ſome days, in ſmall quantities, as an alterative.

I ought to obſerve, that the bark ſhould be freſh-powdered, and the electuary new made every day or ſecond day, otherwiſe the bark loſes its auſtere but grateful bitterneſs on the palate, and its proper effects on the inteſtines.

V. *A Gangrene ſtopped by the* Cortex Peruvianus; *by Mr* SAMUEL GOOLDEN *Surgeon at* Bridgnorth *in* Shropſhire.

I Cannot help not only expreſſing my own pleaſure and ſatisfaction, but muſt alſo congratulate every profeſſor of phyſic and ſurgery, upon the laudable attempt of your ſociety, in their endeavours to improve medical knowledge, by their annual eſſays and obſervations: A work which I have long deſired, and thought wanting in our dominions, and do think it a duty incumbent on every profeſſor of phyſic, or any branch of it (in theſe countries eſpecially) to communicate to them any remarkable obſervation that may fall under their cognizance, and may tend to promote ſo uſeful a deſign.

Since that valuable medicine the *cortex peruvianus* has been ſo ſucceſsfully given in mortifications from an internal cauſe, by Mr Ruſhworth, Mr Amyand, Mr Bradley, Mr Douglas, and Mr Shipton; a remarkable inſtance of its ſucceſs, in a more than parallel caſe, I had lately the pleaſure of obſerving in a patient of mine. I have not the vanity to think that what I can ſay will add any thing to its credit; but it may be a means to make its virtues in ſuch direful diſorders more generally known, and help to confirm what the above gentlemen, and eſpecally Mr Douglas and Mr Shipton, have ſo ingeniouſly ſaid of it.

*January*

*January* 8. 1733-4.

Samuel Lewis, aged 76, of a pale complexion, and choleric conſtitution, a luſty, and ſeemingly a very healthy man, having but little ſickneſs from his youth, ſhewed me an inflammation of his left leg, extending from an iſſue he had below his knee, down to his ancle, and all round his leg, partaking of an eryſipelas and oedema. I threw out the pea from his iſſue, and endeavoured, by diſcutient fomentations, embrocations, and cataplaſms, with bleeding and lenient purges, to mitigate the inflammation; but to no purpoſe; for I found it tending very faſt to a gangrene. His leg, from an intenſe red colour, turned livid, black bliſters aroſe, &c. I would have ſcarified it, but was not permitted.

On the 13th day, the tumor was ſunk, his leg black and dry, his pulſe quick, with frequent intermiſſions, his countenance wild, his tongue hard, parched, and dry. He would not permit the neceſſary inciſions to be made. With the conſent of Dr Anthony Weaver, a gentleman of great charity, humanity, and learning, I prepared eight of the following draughts.

℞. *Cort. peruv. opt. pulv. drach. ſem. aq. ceraſor. nigr. unc.* i. *ſem. ſyrup. croci unc. ſem.*

*M.* One of which I gave him about noon, and ordered that he ſhould continue to take one every four hours.

14. About ten this morning, by which time he had taken three drachms of the bark, I found his tongue moiſt, his countenance not ſo wild;

and

and examining his leg, found it impoſtumated, from a little below the ſuperior tubercle of the tibia down to the ſmall of his leg, a little above which I ſaw a ſmall aperture, with a little matter ouzing from it. I told him he ſtood a very fair chance for his life, if he would ſubmit to the ſuitable means. With his allowance I immediately entered the probe-point of my ſciſſars at the aperture, and cut upwards as far as it was hollow; then turned them, and cut downwards as far as the cavity ran, and diſcharged between three and four ounces of a well digeſted pus, and, after fomenting very well with a decoction of the warm plants in a ſtrong lixivium of woodaſhes, *ſal ammon. com. & ſpir. vin. camphor.* (which I had uſed from the time I ſuſpected it would mortify) I dreſſed the inciſion with *ung. baſilic. liniment. arcæi a a. part. æq.* ſpread upon a doſſil dipt in hot *ol. terebinth.* with a cataplaſm of *oat meal, flor. centaur. chamæmel. a a. p. æ.* with the fomentation; and *ol. chamæmel.* over all. He found an agreeable warmth about his leg after the dreſſings were applied.

15. I found him very chearful, and diſcovered a large ſinus betwixt the ſolæus and *gaſtrocnemius intern.* I laid it open, and diſcharged about the ſame quantity of well-digeſted matter as yeſterday. There was a very large ſlough in the former inciſion, which I cut off, and dreſſed as before.

16. He had been very reſtleſs all night, his pulſe irregular, his tongue rough and dry, with fluſhings in his cheeks. Inquiring if he had taken his draughts regularly, I was told he

he had not, through the attendant's drowsiness. After reprimanding them for that neglect, and cautioning him about it for the future, I opened his leg, and found the discharge large, a fungous flesh rising in the first incision, which I sprinkled with *præcipit. rubr.* and dressed as before; and, by reason he had not a stool since the 14th, I ordered him a common clyster, which brought away some hardened excrements. At night his heat and flushings were not so great, and his tongue was moister.

18. Being wearied of his draughts, ordered thus,

℞. *Cort. peruv. opt. pulv. unc. sem. confec. alkerm. unc.* i. *M. divid. in bol.* viii. *cap. unam quarta quaque hora superbibend. cochlear.* iii. *julap. sequentis.*

℞. *Aq. lact. cerasor. nigr. a. unc.* iv. *rut. unc. sem. epidem. unc.* ii. *tinct. croc. in aq. theriac. fact. unc.* i. *confect. alkerm. unc.* ii. *syrup. cariophyll. unc.* ii. *M.*

I observed matter lodged in the *gastrocnem. intern.* almost to the back of the leg; I opened it in the most depending part, but had not the discharge I expected.

21. Compresses and bandage were applied to unite that cavity, and prevent the matter from lodging in it.

22. A sinus running towards the small of his leg opened.

23. He complained of a pain in his side, and had a restless night: I dressed the ulcers only with dry lint; the cavity above-mentioned inclined to unite.

24. He

24. He was very much dejected, but I could not apprehend the reason of it; every thing appeared in good order.

25. He shewed me a swelling in his groin, with great hardness and inflammation down the fore and inside of his left thigh, extending to a pretty large and insensible tumor, a little above his knee, which he found gradually to increase since the 15th instant, but did not speak of it before, lest he should (as he expressed it) be cut there. I applied an emollient plaister over it, was apprehensive he would have a very large abscess, which would exhaust him. There was very little discharge from his leg.

30. To this day, his fever increased, with an irregular pulse, great drought, dry tongue, &c.; notwithstanding he continued the use of the draught or bolus as before, very little discharge from his leg; the ulcer appearing livid, I fomented well, and applied the warm digestive as above. The swelling in his groin very much increased; the inflammation decreasing, I felt matter to fluctuate, but deep; the tumor not very painful. Not having a stool for several days past, I gave him a lenient purge, by which he had a very large stool of black and very fœtid excrements.

31. The hard insensible tumor above his knee of a livid colour, and that in his groin rising towards a point near the inguen, inclining to the inside.

1. 2. February, Instead of matter, there was a discharge of clotted blood from his leg: I dressed it with the warm digestive.

3. The pus was laudable, the tumor in his

groin considerably raised; he took a lenient purge, which gave him one stool, not having one since the 30th ult.

11. To this day, his fever continued, but not in any great degree, and his pulse irregular; a white pustule appeared upon the most prominent part of the large tumor in the inguen, which I cut, and then, entering the point of my probe-sciffars, cut about an inch in length near his groin; well digested matter gushed out, as from a cock, and in as full a stream, sometimes streaked with blood. I took thence at least ℔ iii. His leg begins to cicatrize.

12. A large discharge about the bed from the last incision, and a large quantity of matter that fell below the orifice; yet, in the cavity on the inside of the thigh, I applied a caustic on the lowest part, and discharged thence about *lib. sem.* I also opened that tumor near his knee, and discharged *unc.* i. of well digested pus.

19. I opened another sinus on the inside of his leg, and discharged thence only several clots of blood. From this time the discharge from his thigh gradually lessened; that very large sinus united, by means of compress and bandage; his fever left him, and he did not use his medicine since the 14th instant, in which time, in draughts and boluses, he had taken between *unc.* x. and *unc.* xii of the cortex, which, being continued so long and regularly, I believe assisted nature to expel her enemy in that very large abscess in his thigh, which otherwise might, notwithstanding the mortification was

kept

ſtopt in his leg, have ſeized it again, or have fallen upon ſome more noble part, and occaſioned his death; after which I made him a decoction of the moſt agreeable bitters, by which means he recovered a good appetite; and in a ſhort time was able, with a little aſſiſtance, to walk down ſtairs, and any where elſe in the houſe with a ſtaff only; and, on the 25th of March, he walked to my houſe to be dreſſed, which is near a quarter of a mile; and about a week afterwards went to his work as uſual, (which is mending ſhoes) his leg giving him very little diſturbance: In the day it ſwells conſiderably, but, when he riſes in the morning, is of its natural ſize, for which I ordered a laced ſtocking. His thigh is ſtrong and firmly cicatrized, as alſo is his leg; and the man enjoys good health, and is every way as fit for his work as he was before his illneſs.

### VI. *A mortification cured by the* Peruvian *Bark; by Mr* John Paisley, *Surgeon in* Glaſgow.

*Gentlemen,*

THE good effects of your generous concern for the welfare of mankind are now conſpicuous in the many uſeful obſervations and eſſays that have been communicated to the public in your collections, which, without this opportunity, would probably never have ſeen the light. At the ſame time, I muſt think we are alſo greatly indebted to you for the judicious abridgment of the moſt conſiderable improve-

 ments

ments and discoveries in physic made through Europe, which most private persons remain entirely ignorant of, or it is very late before they are informed of them. It is owing to your second volume that the specific virtue of the *cortex Peruvianus*, in curing mortifications, was known early enough here to be put in practice in the following case.

A surgeon of this place, of a very bad scorbutic habit of body, about forty years of age, had a little pimple on the middle of the underlip, which his barber cut the top from in shaving him on Saturday the 9th of February last. The following evening, upon going out to the cold air, the pimple swelled, and turned hard, with an inflammation all round it; which increasing the Monday following, he applied an antiphlogistic fomentation with spirit of wine camphorated. Notwithstanding the frequent use of these for four or five days following, and his being twice blooded, the inflammation, hardness, and swelling, increased considerably, extending itself to the angles of his mouth, and some way along the cheeks, and all around the chin, with great pain, and with vast disorder through his whole body.

On Friday the 15th, at eleven at night, a small black spot, about the bigness of a herring scale, appeared (not where the wound was, but) on the middle of the red part of the lip, which spread so fast, that, by eleven next forenoon, it covered near one half of his lip that then began to stand out very much, when a consultation of almost all the physicians and surgeons in town was called, who

who advifed the continuation of the fomentation and fpirits as before, and a decoction of the woods. For two or three hours, the mortification continued to fpread, till it had covered almoft his whole lip, reaching inwards and downwards to the gums, the hardnefs and fwelling of the neighbouring parts increafing. Upon this, he was advifed to try the powder of the *cortex peruvianus*, half a drachm for a dofe. He took the firft dofe betwixt three and four o'clock afternoon, and his lip was dreffed at ten at night, when the mortification did not appear to be increafing, at leaft the increafe was very inconfiderable: He then took another dofe of the bark. Towards the morning of the 17th, his lip was again fomented, and he took a third dofe of the cortex. At ten of the forenoon, I dreffed it, and found the mortification had made no further progrefs fince laft night. At night, I dreffed it again, and then, for the firft time, obferved fomething like an appearance of fuppuration at the place where the wound or rather pimple was, but none at all on the mortified part. That night he took another dofe of the cortex, and continued to take two dofes, one in the morning, and another in the evening for two weeks.

The fomentation and fpirits being applied twice a day, and a little emulfion given him for drink, without any other medicine than the cortex, the fuppuration fucceeded well in the mortified parts on the third day after he began to take that medicine; upon which proper digeftives and other dreffings were applied,

plied. The ſloughs caſt off very well, the hardneſs and ſwelling went away, and in 12 or 15 days the lip healed up, though with a conſiderable contraction by the great loſs of ſubſtance.

In very cold air, he ſtill feels a pain in his lip: This, I am apt to believe, does not ſo much proceed from the callus, as from his lip preſſing upon the fore teeth which are very rough and looſe; and which it does more eſpecially when he attempts to ſpeak, by the lip being ſo much contracted.

I have read this account to the patient, and had his approbation of my relation of the facts, which my attendance on him all the time of this diſorder gave me ſufficient opportunity to obſerve.

VII. *Remarks on Chalybeat Waters; by* ALEX. MONRO *Profeſſor of Anatomy in the Univerſity of* Edinburgh, *and* F. R. S.

THE ingenious account of ſeveral ſteel-waters in ſome of our northern counties, given by my worthy friend Dr Thomſon, phyſician at Montroſe, [See *Art:* 6. of our 2*d Vol.*] raiſed a deſire in me to be informed of the moſt remarkable mineral waters of that kind which are to be met with ſo frequently all through Scotland; and, as my view was only to know ſo much of them as was neceſſary in practice, I propoſed to diſcover, by my friends and correſpondents, what their real or comparative ſtrength was, and how well they would carry and preſerve without loſing their virtues, whereby

by physicians might judge which of them was most proper in the various diseases and circumstances of patients, and which must be drunk at the fountain, or would serve as well when kept.

It was necessary in such an inquiry that there should be some general method by which all the trials should be made, otherwise there would be an insuperable difficulty in making the comparison of them. The writers on this subject have contented themselves with telling us, that such waters strike a red, purple, violet, or black colours, when galls or such other astringents are mixed with them, and that this change of colour is a sure mark of a chalybeat water; and some have said, that the deepest colour shews the greater proportion of steel. To satisfy myself of the truth of this, I dissolved artificial *sal martis* in a small quantity of fountain water; and then, dropping more or fewer guts of this solution into a given quantity of common water, I found that, by the mixture of the tincture of galls, I could form all the different colours mentioned, the larger quantity of the solution always requiring the greater number of drops of the tincture to bring it to all the colour it would take, and that being as constantly deeper than the others, where fewer drops of each had been employed. If words could express the numerous degrees of colours between the pale-red and the black, the lightest and deepest of these mentioned, the simple experiment of bringing steel-waters up to the deepest colour they could strike with galls, might be sufficient to deter-
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mine the different proportions of ſteel in each; but, as the variety of colours cannot be expreſſed in words, and I wiſhed to come to the knowledge of nearly the real quantity of ſteel contained in any given quantity of each water; there was a neceſſity of having ſome general ſtandard to which all might be brought. To obtain this, I obſerved the quantity of ſteel employed in preparing *ſal martis*, ſaw how much ſalt was got, what the reſiduum of earthy parts was, made a ſmall allowance for ſome evaporating during the effervescence of the *limatura martis* with the oil of vitriol, and concluded that the proportion of ſteel in the artificial ſalt or vitriol of iron, was very little more than a third part. Next, I diſſolved a certain quantity of this *ſal martis* in fountain water, weighed the powder that precipitated from it, weighed the whole ſolution, and then, putting ſome of it into a ſmall glaſs, I dropped it *guttatim* into another glaſs, counterpoiſed exactly in a ſcale, till I ſaw how many drops of this liquor weighed two drachms; after which, by common arithmetic, it is eaſy to know how much ſalt, and conſequently very near how much ſteel it contained in any given number of ſuch guts. To ſave my friends the trouble of making ſuch ſolution, I prepared 20 ounces, which contained an ounce of the *vitriolum martis*, except a ſcruple which was precipitated, and 142 gutts of this ſolution weighed two drachms; therefore every ſuch gutt contained $\frac{1}{23}$ of a grain of ſalt, or $\frac{1}{69}$ of a grain of ſteel. The difference of the bulk of the drops let fall from different glaſſes ſhould

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not, you fee, at this rate, make a very confiderable error; but, to prevent this as much as I could, I chofe all the glaffes as near to the fhape, fize, and thicknefs of the lips of the one I firft ufed, as I could get them.

To make a comparifon then of any chalybeat water with this folution, into a determined quantity of fuch water, pour drop after drop, of a ftrong clear tincture of galls, allowing a fufficient time between each drop, for its having its full effect, till it is obferved that the addition of more tincture makes no change on the colour of the water; and, to make fure of the number of the gutts of the tincture that are requifite, let the experiment be repeated feveral times. Then having the fame quantity of common water as was employed of the mineral water, in a glafs of the fame dimenfions, thicknefs, and tranfparency, with that made ufe of in the preceeding trials, pour into it the number of drops, difcovered by them, of the fame tincture of galls, and mix them well; after which, in the fame cautious manner, drop in the folution *guttatim* till their colour is the fame with that of the mineral water. When once the quantity of folution, equal to the contents of the fpaw, is known, pour a due proportion of it into common water, and let feveral people examine whether the tafte of it is not the fame with what the mineral water has. I have thus made fountain water fo like to feveral chalybeat waters, that none could diftinguifh them.

I prefer tincture of galls to their fubftance for making the foregoing experiments, becaufe it

it produces its effect much fooner and more equally than the powder, and a lefs proportion of the virtue of the galls can be added at once; which, from an obfervation communicated to me by Dr John Taylor, phyfician here, and verified by me afterwards in feveral fteel-waters and in common water impregnated with *fal martis*, would feem very neceffary to be regarded; for, if too large a proportion of galls is at once poured into fuch waters, for example, if 60 or 100 drops of a tincture of gall is thrown into a water that requires only 8 or nine to bring it to its deepeft colour, it will be fo far from making it ftrike the colour ftronger, or fooner, that, for feveral hours after, no change will be obferved on the water; and at laft it gradually becomes of a deep fea-green colour, inftead of the purple or violet it would otherwife have turned into. It is not impoffible that good chalybeat waters have been fometimes condemned as containing no fuch mineral, or of being impregnated with copper by a mixture of too much galls at random.

Recent tincture of galls is certainly preferable to that which is long kept; but, I can affure you, that, after I had kept fuch tincture till it was covered with a thick mofs, or was mouldy, as we call it, a-top, and had a vifcous thick fediment, it ftill produced the common effects on fteel-waters.

I would propofe, that the comparifon above defcribed fhould be made with frefh water at the fteel fprings in different feafons, and fhould be repeated once a week with water that is right put up in bottles, well corked and fealed, in

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theſe different ſeaſons, till it becomes vapid by keeping; by which the proper ſeaſons for bottling the mineral waters, and the time each will preſerve, can be much more exactly known than it is poſſible to determine by the ſmell and taſte of them.

It will be alſo requiſite to obſerve what time it is before the galls ſtrike the colour fully after they are put in, and to remark how long this colour remains in an open glaſs; for it appears reaſonable enough to think, with the French academiſt Mr Geoffroy *, that more time will be neceſſary to ſtrike the colour fully, but that it will remain longer in ſuch mineral waters where the ſteel is moſt intimately diſſolved in, and blended with the other principles.

To make the account of the ſpaws compleat, their other contents ought to be ſought after, by mixing different ſubſtances with them, remarking the changes they undergo in ſmell, colour, *&c.* by keeping, and by extracting their ſalts and earths after evaporation.

By theſe means it is, that I was in hope to have furniſhed you with a pretty compleat liſt of the moſt remarkable ſpaws in this country, with the comparative ſtrength of each, and the time they kept; but, being diſappointed of ſeveral I expected, while informations of others I had not heard of are often brought me, I ſhall reſerve what I have received, till I can preſent you a more full account, which your publication of this invitation will probably

* Memoires de l'Acad. de Sciences, 1724.

bably ſoon enlarge, by acquainting the gentlemen who have the opportunity of examining ſuch waters, of the common method they ſhould all take in their inquiries, and to whom they may addreſs their diſcoveries. In the mean time, allow me here to annex ſome experiments, which thinking on this ſubject led me to make.

I had obſerved, as above, a ſtrong reſemblance between our ſteel-waters and common water in which a ſmall quantity of *ſal martis* had been diſſolved; but the natural ſpaw waters, when expoſed to the open air, very ſoon loſt their chalybeat taſte, and would not ſtrike a purple colour with galls: When expoſed to heat, their virtues were much ſooner loſt, and in the cloſeſt veſſels they, in no long time, became vapid: Whereas *ſal martis* bears heat, and being expoſed to the air, without perceptibly loſing any thing. I ſuſpected this difference might in part depend upon the ſmaller proportion of the vitriolic principles in the chalybeat waters, and ſome change they might thereby undergo in the water; and, therefore, having added to ſome bottles of water as much *ſal martis* as had been found to make it of the ſame taſte, and to ſtrike the ſame colour with chalybeat waters, I corked ſome carefully up, others I put bad corks into, and a third ſort I left open. The water in theſe laſt loſt its taſte and virtues in about a fortnight, with little other change than becoming a little muddy, and having a ſaffron-coloured powder at bottom. The ſecond kind

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kept some time longer, but had a little of a ſtinking ſmell before it became vapid. The water that was carefully corked and roſined, kept well, but acquired a pretty ſtrong ſmell of rotten eggs, exactly like to what ſeveral ſpaws had when kept; and, when the bottle was left open, the ſtinking ſmell went ſoon off, and ſoon after the chalybeat virtues were not to be obſerved, and the bottom of the bottle was covered with the ſaffron-coloured powder, which is generally to be ſeen alſo in bottles where natural chalybeat waters have been kept any time. So far therefore the difference between the natural and artificial ſteel waters ſeemed only to be in the greater volatility of the natural ones.

Several of our own chalybeat waters, and the two foreign ones of greateſt reputation, Spaw and Piermont, ſeemed to me an exception to the reſemblance I had found between the diluted ſolution of *ſal martis* and the natural ſteel waters; for the colour they ſtrike with galls is very faint, while their taſte and other effects are remarkably ſtrong. This put me on trying to find ſome artificial way of imitating them: For this end, I mixed filings of iron, oil of vitriol, and water in a florence bottle, which I laid on its ſide, and immediately fitted another to it, in which I had put ſome fountain water. The fumes that roſe upon the effervеſcence of the oil of vitriol with the ſteel, came over into the other glaſs. After the violent effervеſcence was over, I took away the glaſs with the water, which was quite limpid, but had a ſtrong empyreumatic

ſmell; its taſte was pungent at firſt upon the tongue, and then the acidulous taſte prevailed. When tincture of galls was mixed with it, it became of a red-purple but faint colour; which held a great many days without any obſervable precipitation. Next morning, the empyreuma of the remaining water was gone, and it had a very agreeable briſk ſpaw taſte: In leſs than a day after, this alſo went off; a ſmall quantity of the ſaffron-powder was fallen to the bottom of the glaſs, and the galls had no effect on the water.

On a ſuſpicion that this artificial water would give evident ſigns of acidity, whereas the ſolution of *ſal martis*, and ſome of the ſpaws that ſtrike a black colour, rather appear alcaline, by changing ſyrup of violets and clove-july-flowers into a green colour, I mixed it with theſe ſyrups, without changing the one into red, or heightening the colour of the other. It is true, it did not make them green, neither do the more ſpirituous ſpaws.

The ſucceſs of this experiment, which was beyond my expectation, led me to try if I could not diſcover what it was that evaporated, and what was precipitated in theſe waters. I ſuſpected the menſtruum to fly off, and the ſteel to be left behind. To know then if this ſaffron coloured reſiduum was ſteel, I poured oil of vitriol on it, which made no remarkable efferveſcence: I then added water, and let them ſtand two days, when ſome of the powder ſeemed to be diſſolved: Upon mixing tincture of galls with it, the colour did not change; but, upon further addition of ſpirit of

of hartfhorn, a great effervefcence followed, and a fudden coagulum of a deep red purple fubftance was made. This change of colour I at firft imagined to depend on the refiduum; but, upon mixing the other ingredients without any refiduum, the fame coagulum was formed; and therefore, if the powder precipitated from the mineral water is the particles that compofed iron, they are greatly changed. The refemblance of the ruft of iron to this powder, made me try how thefe experiments would hold with it; and they came out in the very fame manner: Nor would ruft, or the powder, when fufpended in water, ftrike any colour with galls, though the *crocus martis* of the fhops did.

What flies off in the evaporation is evidently the menftruum, in which the irony particles were diffolved, which carries away fome of the principles of the iron with it. From feeing the effects of acids on iron, obferving the effects of that almoft univerfal menftruum of the air upon this metal, and finding vitriol of iron naturally formed, we have reafon to judge that the menftruum of the irony particles in chalybeat waters is alfo an acid. And, confidering how much fulphur enters into the compofition of iron, what a ftrong ftinking fmell (which can only depend on fulphur) mineral waters have before they turn vapid, and how much the fulphur of iron is deftroyed before the iron turns into ruft, which again refembles the powder precipitated in thefe waters; all this would perfuade us, that the menftruum carries the

fulphur along with it, and leaves only the earthy parts, with a fmall proportion of fulphur.

May not fteel-waters be impregnated with common grofs vitriol, or with the more fubtile fumes of iron diffolved in the natural menftruum, or with both in different proportions?

Will not the lefs volatility of fome chalybeat waters, and the quicker precipitation of their irony particles, after the mixture of galls, fhew the fulphur not to be fo much freed from the earthy particles, as in others that precipitate more flowly, and are more volatile?

Will not the common obfervation of air generated in all effervefcencies, fermentations, putrefactions, and where-ever a confiderable change is produced in the compofition of bodies, account for the quantity of elaftic air, obferved in chalybeat waters, in the more fpirituous when recent, in others when the putrid fmell fhews the fulphur to be more difengaged?

Are not the different kinds of fteel-waters to be prefcribed, according as there is occafion for a fubtile penetrating fulphureous fpirit to pervade the fmalleft veffels, or according to the quantity of an abforbent aftringent earth that is required to be joined with this fpirit?

Where different fpaws are not to be had, may not the fame water be made to anfwer each of thefe intentions, according to its being more or lefs kept, or expofed to the air or heat?

VIII. *An*

VIII. *An Inquiry into the Mineral Principles of* Montrose *Water; by* ALEXANDER THOMSON, M. D. *Physician at* Montrose.

AFTER all the inquiries made into the contents and natural appearances of mineral springs in Germany, France, and England, by men most accomplished, and means promising the best success, the subject seems yet much in the dark, when all appearances are justly weighed.

Of the latest inquirers into the English waters is Dr Short, and after him Dr Shaw has examined the Scarborough water particularly. They have entered more into the subject than most others before them, and offered fairer at a discovery of their virtues from their real principles; yet all the learning of the first, and accuracy of the other, have still left the subject involved in variety of doubts, of which I shall only mention a few.

That spaws strained through mineral bodies, dissolvable in water, may not, and at times actually do not partake of all, but only of some of them, as Dr Short hath observed of the allum stone in the clifts of the rock through which the Scarborough water strains; and that yet the water gives no vestige of allum, upon all his trials.

From the same instance of allum, he hath, I think, observed something more mysterious in the composition of mineral waters, to wit, That, in the congeries of mineral bodies through which they flow, two of different natures,

natures, as allum and fulphur, may be fo involved, that the one apparent as allum, gives, upon trial, a demonftration of the prefence of the other, whereof nothing appeared before; as in the folution of the above-named allum-ftone, both the fludge, or undiffolved dreg of the ftone, and alfo the fediment of its folution, give an intolerable fœtor, as ftrong as that of the Harrogate fulphurous well, and, in a red-hot iron fpoon, throws out a blue flame, whereby fulphur is known to be diftinguifhed.

That the purple tincture of fteel fpaws, with galls and fuch like, fhould be owing to the vitriol of chalybeat waters, and notwithftanding what gives fuch tincture fhould inevitably fly off lefs or more, or altogether, an hour or two after drawing, or fooner by feveral degrees of heat, the higheft within the boiling degree, when nothing in vitriol that gives fuch tincture hath been obferved to fly off in that manner.

That the medical effects of mineral waters in general, in cafes of the fpirits or animal faculties affected, fhould be generally obferved, and particularly by Dr Short, to be beft when drunk at the fountain; and yet that Dr Shaw fhould find, that the falt of Scarborough water, got by evaporation, cheared the fpirits, and in general gives life and frefh powers to nature, as much as the water when drunk frefh at the fountain.

That a mineral water fhould be 17 or 18 grains a pint heavier when carried to a fmall diftance, than when immediately drawn, as Dr Short hath obferved of Scarborough water,

ter, and this ſhould be owing to a more dilated air, or a very ſubtile mineral ſpirit; and yet no air for being more dilated is leſs a body, neither does any mineral ſpirit, for being ſubtile, become no body, and therefore not ponderous. Bodies differ from bodies in their ſpecific gravities; but no ſubtilty or dilatation hinders every body from gravitating: And therefore, whatever it be that evaporates from the water, it ſhould, by all that is known of bodies univerſally, rather diminiſh than augment their weight by its abſence. Nor would there be leſs difficulty to conceive a reaſon why the weight of the water thus increaſed in ſo ſhort a time, ſhould increaſe no more a day, a month, a year after. All this notwithſtanding, the Doctor hath likewiſe obſerved, that the chalybeat Scarborough water weighs ſtill heavier the more it is freighted with this mineral ſpirit.

The general uncertainty wherein the greateſt naturaliſts, as Mr Boyle and Mr Hoffman, have put or left us, on the ſubject of mineral ſprings; and the more particular difficulties I have now choſe out from among ſeveral others, which, for brevity, I omit, wherein the two learned phyſicians I have named, after their more accurate diſcoveries, have left us yet involved; and the laws of inquiry, propoſed by the lateſt of them, requiring an age, or it may be ages, rather than the endeavours of any one, or of a few men: Theſe conſiderations, I ſay, are a ſufficient caution to me, not to be too deciſive in a matter ſtill ſo obſcure.

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The MONTROSE-WELL is situate in the richer sort of our soil; the upper lay being of a blackish or moss-colour; the lower, to about three feet depth from the surface, lays interchanged perpendicular, one of soft clay, the other of sand, soft, and as it were fattish to the touch; through which lasts at the above depth, the springs drill out horizontal to the surface in several parts.

The water, when new taken up, is of a whitish colour, above that of ordinary spring-water; the taste soft, and faintly discovering the mineral quality. I compared the weight of this water with two of the most noted of our steel springs, and one of fine ordinary spring-water, in a balance that was turned by one grain, and could observe scarce any difference of weight in any of them. This difficulty I cannot well account for.

I began my trials on this water with those commonly made with steel springs, but they did not answer; and I suspected its purgative quality on the drinkers might be owing to its being generally drunk very muddy, by people's thronging on it, till it was built about. A fews days after, by an accident, I began to conceive it to be of a different nature from the steel springs. A phial-glass into which several parcels of the water had been thrown at random, with several things, as galls, rose-buds, and green tea, being broken by accident at night; in the morning, I perceived a white milky substance covering the floor where the glass was, and about it a scurf variegated green and other colours, much like these

thefe of a peacock's tail. Thefe led me into a fufpicion of fomething fulphureous in the water.

I dropped fome gutts of *ol. tart. per deliq.* into a glafs of the water, and there prefently formed at the bottom of the glafs a white light equable cloud, [illegible] perfectly as appears ordinarily in the urine of people in fevers, at the time when they have a favourable crifis, or immediately after it, the one and other waving with a plain equable furface, by inclining the glafs to and again, only that of our water was of the pureft white. As this appearance in fuch urine gives, as I imagine, plain notice of the morbid fulphureous parts of the blood now perfectly concocted, and fo washed off by the urine, as by the other ftrainers of the body, the analogy feemed to me natural betwixt the two appearances in the urine and in our water.

To try this further, I made *fulphur folubile cum alkali fixo* after Boerhaave's way, four parts fulphur to a fifth *fal tart.* This diffolved in water, and *ol. tart.* dropped on it, gave a cloud, but lefs decernible than what appeared in our water, which I imputed to the yellow tincture of the folution not reflecting the rays fully: And accordingly a day after the *fulphur folubile* having become moifter by the air it imbibed, and turning whiter, I diffolved it again, pouring on water till the folution became of the colour of our water when new drawn, and the *ol. tart.* affufed gave confpicuoufly the fame manner of cloud with that in the mineral water, fo that the one could fcarce

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or not at all be diſtinguiſhed from the other.

The ſucceſs of theſe experiments, compared with the above accident, made me incline to believe that theſe clouds were lac of ſulphur. To make trial of this further, I would ſee what a ſtrong aſtringent ſalt would do with the water of our well; I choſe allum, and, weighing a ſmall piece of it, I put it whole among ſome of the water, and, by conſtricting its pores, (as I imagined from the allum's having loſt none of its weight when taken out of the water,) it quickly formed a white coagulum, which, by mixing ſome drops of *ol tart.* preſently falling to the bottom, I poured off the water, and evaporated what remained by the heat of the ſun, into a concrete maſs, of a ſoft taſte, and with nothing ſaltiſh therein.

For trial whether the effects of the alkali ſalt might be the ſame, or near ſo, on ſulphurs univerſally, whether volatile or fixed, I dropt ſome guts of *ol. tart.* on common ſpirit of wine, and preſently had an equable cloud formed towards the bottom of the glaſs, and of higher colour than the liquid above. Juſt ſo when it was infuſed on *ol. oliv.* it formed a cloud or lac of an equable ſurface to the bottom; for the oil above poured eaſily off, the conſpicuouſly white lac remained. The ſame ſucceeded with the ſolution of *ſal tart.* among water poured on theſe liquors.

As I here only conſider the analogy of things, I have not to do with what is called the *rationale* of them, or to account how the alkali, which diſſolves ſulphur, or renders it ſoluble,

luble, should in such manner appear to precipitate it, by separation of parts, an effect rather belonging to acids, as in making the *lac sulphuris*: Neither will the narrow bounds I am confined to allow me to enter on such inquiry; and the rather that it will further appear, that chemists are yet much in the dark concerning the phenomena of these famed salts, or what are generally reputed such.

I went on next to try what appearances spirits, both the alkaline and acid, could make on our water.

By spirit of hartshorn, there quickly rose atop of the glass an equable cloud all over the water, resembling much the colour of old brandy, which, in about half an hour vanishing, left the water more milkish, and as it were troubled. This seemed to answer well enough to the former experiments.

Spirit of nitre turned its native milky colour into a fine light-blue, from the top to below the middle of the glass, and, as it stood longer, the colour advanced further downwards.

Spirit of vitriol changed the whole body of the water into a faint purple, or dilute pink colour.

As I could make nothing of the two last trials, I tried next solution of sublimate corrosive; and by some drops thereof on our water, the same manner of cloud began to form immediately, but formed out more gradually.

The same solution, on solution of *sulphur*

cum

*cum alkali fixo*, made prefently a coagulum, but no equable cloud.

By thefe, with the two former, it appears that the alkali prevails in the falt of our water: But this can determine nothing concerning the fpecies of the falt; as it will appear, that even the moft reputed acids of the foffil falts prove no lefs alkaline upon trials.

But what comes next to my prefent purpofe, is, That the analogy hereby appears of the effects of the ftrongeft alkalies and ftrongeft acids, both agreeing in forming an equable cloud in our water. And I conceive the coagulum formed in the folution of *fulphur cum alkali fixo*, to have arifen from the cloud being confufed by the mercury being more difengaged of its acid menftruum, by a ftronger alkali than is the falt of our well, and thereby precipitating in greater quantity.

I found by the above trials on Scarborough water, the analogy and differences following betwixt it and our water.

*Ol. tart.* affufed on it, formed immediately a thick milky cloud ftanding equable in a minute or thereabout.

Solution of fublimate corrofive on the fame water formed immediately a thin white cloud, rifing to the middle of the glafs.

Solution of fublimate corrofive on our water gives juft fuch a cloud as *ol. tart.* on Scarborough water; and, when it begins to diffufe itfelf, the affufion of a little more of the folution makes it become again more confpicuous.

Solution of *fal tart.* forms the cloud in

Scarborough and our water alike; with this difference, that in the Scarborough it formed more immediately, in our water more gradually, but standing longer without diffusing.

I made a lac of the Scarborough water in the same manner, and answering to the same sensible qualities as that made of our water.

The lac may be made of both also, by a piece of a gall entire, or any such like astringent, and *sal. tart.* but not so pure.

Powder of galls mixed with both, gradually turned both into a dusky green, with foliages suspended through the waters, in irregular planes.

These, I think, are as near resemblances as are readily to be found betwixt one water of the mineral kind, and another; and any variety that appears may be owing to some variety in the salts of the one, not so much, or at all, in their nature, as in their combination, or in some difference of proportion. To know what account to make of these, and whether any salt by itself, or in combination with others, could come up to the above appearances, I made the following trials.

I made the experiments, as above, on each of the fossil salts, and also on sea-salt dissolved in common fountain water, first separately, then on their various combinations, as I could conjecture they might most probably prove, or be the contents of our water, but found none except the following come up to such purpose.

*Ol. Tart.* on solution of nitre makes no change of consequence; but, on solutions of nitre and of sea salt mixed, formed, a cloud to

to the bottom; but ſolution of ſublimate corroſive on the above ſolutions mixed made no change, though remaining a conſiderable time in the glaſs.

Solution of ſublimate corroſive on ſolution of nitre made at firſt no change; but ſome minutes after, formed a cloud, as by *ol. tartar.* on ſolution of ſulphur with the fixed alkali, in our water.

Mixture of ſolutions of allum, nitre, and ſea-ſalt, made a troubled white.

*Ol. tart.* affuſed, gave ſome appearance of a cloud, not equable, but interſperſed as in precipitations, and ſwimming above the bottom.

Solution of ſublimate corroſive on the above ſolution, does nothing further than change the ſame into bluiſh.

*Ol. tart.* on ſolution of ſea-ſalt, for a conſiderable time made no change; but then falling down troubled, and making it as it were muddy to the bottom, it gave ſome faint appearance of a cloud, but not ſo equable.

*Ol. tart.* on ſea-water made immediately a white precipitate, then quickly formed a thick cloud, which ſome time after precipitated an equable cloud.

Spirit of hartſhorn on ſea-water formed quickly an equable cloud or ſuſpenſion a-top of the glaſs, about the breadth of a ſmall ſtraw, much reſembling that made by the ſame ſpirit affuſed in our water: But I could obſerve nothing of that whiteneſs nor curdling that Dr Short obſerved to be made by this ſpirit and *ol. tart.* on ſea water. I at firſt ſuſpected,

suspected this suspension a-top of the glass by affusion of the spirit, to be only the spirit a little tinged, geving a-top, and tincturing both our water and the sea-water. To make sure whether it was so, I affused a little of the spirit on good spring-water, and waiting some time, I found no change, and thereby perceived a further improvement of the above analogy, which I shall now state.

By the trials above, it appears that no salts come up to any analogy with our water and that of Scarborough, except nitre and sea-salt; but especially that of the sea, without evaporation, comes nearest. Yet none of them, either by themselves, or in their combinations, come up to them in the trials both with acids and alkalies; whereby it appears that the specific qualities of these waters are not confined to either or both these salts or the earths they lodge in. As on the other hand, these salts being both sulphureous, thereby they the better correspond to a further sulphureous principle in these waters, answering more perfectly in the analogy above stated with sulphur itself. The inflammability of nitre proves its sulphureous kind; and the difference of the salt of sea water before the evaporation from prepared salt, appears plainly above, from the different effects of the fixed alkali on sea-water, and on solution of sea-salt. This difference is owing to the sulphur of the salt in the water before exhalation, as may appear from the volatile alkali casting up the suspension or cloud abovementioned, which it doth not in the solution of sea salt more than in

common ſpring water; at leaſt the one was conſpicuous, the other ſcarce or not diſcernible.

This agrees with, and is confirmed by Dr Short's account of this matter: For he kept ſea water cloſe corked and pitched, with a bladder tied over the neck of the bottle a month; then opening the bottle, expoſed it a few days, then ſhut it up cloſe again: On opening the bottle again after ſome time, he obſerved that it ſtank inſufferably, not much ſhort of the Harrowgate ſulphur-well. The Doctor imputes this fœtor to a high alkali in the ſalt of the ſea; which I do not well underſtand, ſince no alkali, as a pure alkali, eſpecially of the fixed ſort, hath been obſerved to be fœtid without being ſulphureous, as I believe ſulphur hath hitherto been obſerved to be the ſubject or ſubſtratum of all fœtor; and the ſmell agreeing with that of the Harrowgate well, ſhould rather have determined its quality to be of this kind: Therefore I rather would agree with the Doctor's concluſion, that it contains nitre, and aſcribe accordingly the fœtor from its corruption to the known ſulphur of that ſalt; but it is not my buſineſs at preſent to judge in ſuch things.

The laſt thing I have here to obſerve on this part of the analogy is, That as *ol. tart.* on ſolution of nitre and ſea ſalt, gives a cloud, and ſolution of ſublimate corroſive, on ſolution of nitre alone, gives alſo a cloud, I tried to make a lac of both, in the manner I did by our well; but I had nothing of either but a coagulum ſwimming through each, without

out any lac falling equably to the bottom: So that hitherto I have found nothing but of the ſulphureous kind, which gives a cloud; neither can any thing but ſulphur itſelf, it ſeems, give a lac. By a pure accident, I ſaw this further confirmed; one of the mixtures I made of the ſolutions of ſalts as above, particularly of allum, nitre, and ſea ſalt, with *ol. tart.* affuſed, thrown at random into ſoap-water, an equable lac fell to the bottom, leaving the water clear above.

So far analogy hath led me, and I have kept the laws thereof the beſt I could, without ſtraining beyond what nature can eaſily bear. Other ways have been thought on to get into the intimate principles of mineral waters, but ſucceſs hath not well anſwered the deſign; nothing hath yet been found to come over the ſtill, whereby we could have learned more for any real uſe, than we could have attained to without that labour. Chymical mixtures, to diſcover contraries by contraries, alkalies by acids, and acids by alkalies, have as little reached the compoſition of natural bodies, the mineral eſpecially. All the foſſil ſalts, which authors have contended for, as the contents of mineral waters are ſuppoſed acids, and appear ſo more or leſs to the taſte; yet they have been all of them, the moſt acid not excepted, as vitriol and allum, found, by the trials of the learned and ingenious Monſ. Tournefort in his preface to the hiſtory of plants, to go leſs or more on the alkaline nature; as of the moſt alkaline of earths, burnt limeſtone wants not its own acid. So that upon the whole theſe names teach us leſs in

in nature or medicine than is commonly thought; whereas the way of analogy betwixt things known and things fought for, if right purfued, feems the moft univerfal way chalked out by nature for us, in our inquiries of nature; and if it come not up to demonftration, it ftill comes neareft to truth of all the other methods of mens contrivance. The learned have contented themfelves with this method in their inquiries into fteel-fprings. Galls, with infufion of iron, or its vitriol, gives a higher or lower purple: Therefore, whatever with galls ftrikes fuch a colour, is concluded to be of iron-mineral. Why may not the fame way of reafoning on all mineral fprings be allowed?

To know the more adequately to what fpecies of falt or falts that of our well and of Scarborough might be reduced by further trials on them, I was at the pains to compare and mark down the agreement or difagreement in every one of the foffil falts, alfo in fea falt, as they were tried by the learned and ingenious author above named; and from his trials, compared with thofe above, and others to be yet named, on the decoctions of thefe waters, I found the agreement of their falts with nitre and fea falt much nearer than with any other; though there is none of thefe falts but what, by fome trial of the fame author, falls in more or lefs into the quality of the other. The particulars I would have marked here, but that they would have exceeded the juft bounds I ought to confine myfelf to: And, if any have the curiofity, he may fatisfy himfelf, by comparing the trials with Mr Tournefort's book.

By

By the experiments also of the fame accurate gentleman on the natrum of the Levant, compared with these on nitre and fea falt, and all three compared with his chief inquiry concerning the natural falt of the earth, it appears, that the natrum and natural falt anfwer almoft the fame trials, and that the nitre and fea falt anfwer to both more than any other falt; that is, nitre and fea falt appear to make up the greateft part of this univerfal falt. And, if to this we add his difcoveries concerning the natural fulphur of the earth, we may hence probably have the more natural account of fuch mineral fprings formed by the fpring falling in its way through the crannies of the earth, into a congeries or beds of fuch mineral fubftances gathered together, as certainly it is from a greater congeries of mineral particles under or above the furface of the earth, ferving as embryos of concrete minerals, that fuch minerals are formed. I am glad that Dr Short feems to account for the comparative ftrength or weaknefs of one and the fame mineral waters at different times, as well as of two fprings of the fame kind, from fome fuch thought as this. It would alfo hence feem that we need not be over anxious in finding out, or fearching after this or that concrete mineral in the neighbourhood of any mineral fprings, for determining its nature or contents; though ftill the prefumption may be ftrong in fuch cafe, that the one qualifies or affifts the other. But, after all, this prefumption does not come up to certainty; for the fame learned author hath obferved,

obſerved, that, notwithſtanding the allum ſtone in the clefts of the rock through which the Scarborough water flows, yet it hath nothing of allum in it.

I thought it beſt to finiſh this analogical inquiry into our water, and to give it entire, and in its native condition, before I ſhould enter on examination of the contents got by evaporation of the water.

I cauſed to dig as deep as could be well reached, with a ſmall ſhovel, along the bottom of the ſand-lays, through which the ſmall ſprings of the water run, ſeparating ſome pounds of the ſand from the adjacent clay: This I boiled leiſurely in about four Engliſh quarts of the water to a half; then, pouring off the water, I ſtrained it through grey paper. This I put to a ſlow evaporation on the fire, obſerving if any pellicule appeared; but I ſaw none. After drawing off the remaining water with bits of grey paper, and evaporating the moiſture at the ſun, the reſiduum came to about five grains of a black ſubſtance, ſcintillant in thin foliages, and of a fattiſh taſte. I put this in two drachms of ſpirit of wine rectified, and had gradually a ſulphureous greeniſh yellow tincture. I dropt a little of this into a glaſs of water, which ſhewed itſelf quickly a-top, of an equal ſurface; and, after ſome hours advancing downwards, its colour became more dilute to the bottom. Then I dropt on it ſome gutts of *ol. tartar.* which formed a cloud to the bottom, equable and waving

waving by the motion of the glaſs, as above narrated.

Having poured off this tincture, I put on the reſiduum about the ſame quantity of ſpirit of hartſhorn, which drew a black tincture reſembling a bitumen by its thickneſs of conſiſtence. I poured water on it, and it fell to the bottom without mixing with the water: After ſtanding a while, it roſe up again gradually, but did not incorporate with the water. Some guts of the ſolution of *ſublimate corroſive* precipitated it, and let fall a black powder. This, after drying, I caſt into a red-hot iron-ſpoon, and it ſparkled all over; but, caſt after on a live-coal, it ſenſibly flamed blue. I threw the remaining reſiduum three ſeveral times after into the red-hot iron ſpoon; it ſtill ſparkled as before, remained black, and its ſcintillant particles ſhewed themſelves as at firſt. To try further what the heat of a furnace could do, I put it into a crucible placed about five minutes in the moſt intenſe heat of a goldſmith's furnace, and got returned me near one half, after it had ſtained the bottom of the ſmall crucible with an indelible black colour.

I tried next, whether the lac I had made as above would prove inflammable by putting it on a fire in an iron-ſpoon, but I had no fuſion nor flame by the heat increaſing; on the contrary, when the ſpoon became red, my lac turned into a black cinder; but having thrown ſome of the lac into the ſpoon firſt made red-hot, it preſently took flame, and continued ſo till it burnt into black aſhes.

I would next try what a heat increaſing leiſurely would do with ſome *lac ſulphuris* I had lately made with the *calx viva*; it did not flame in the red-hot ſpoon, but burnt to aſhes. I tried the ſame with the officinal *lac ſulphuris*; but neither did it give any flame till the fire was too haſtily increaſed: But then, the ſpoon being taken from the fire, the flame ceaſed; and, the ſpoon being put on the fire again, the lac burnt alſo into aſhes without flame. I tried alſo ſome ſulphur of Aix in the ſame manner, and the ſucceſs was the ſame, it being reduced into a red cinder, which gave no manner of flame when thrown into the red-hot ſpoon.

Theſe things, it may be, are paradoxes concerning ſulphur; the whole whereof is believed to be inflammable, in whatever manner it is treated on the fire.

Some time after, I tried in the red-hot ſpoon a lac I had made of Scarborough water; but it flamed not openly, though put on the fire; only in about a minute it ſhined above the brightneſs of a live-coal, and reſembled a phoſphorus. Having at the ſame time by me a lac I had formerly made of our water, I treated it in the ſame manner, and the appearance was juſt like to the other, both continuing in that bright ſtate a pretty good time before they were reduced to a black cinder; whereas, when the lac of our well broke out into open flame, it quickly changed into a cinder. I tried, in the ſame manner, a little of the reſiduum of Scarborough water after

evapo-

evaporation, much reſembling in colour the lac of both, and the ſucceſs was juſt the ſame.

Though I think this appearance on the fire demonſtrated a ſulphur in the contents of both waters, I had various conjectures why the lac of our well had not anſwered to open flame as formerly; I afterwards thought on yet a ſurer way to try the inflammability of the contents of both. I cauſed a crucible to be made red hot in a goldſmith's furnace, then, having removed all flaming coals from the neighbourhood of the crucible, that any flame without might not confound the appearance within the crucible, I caſt ſome of the reſiduum of our well into it; and ordering to puſh the fire by blowing the bellows, I ſaw the flame within the crucible gleaming, then filling the crucible, and continuing ſo a conſiderable time, the bellows ſtill going. Then, having emptied the crucible of the reſiduum, I tried in the ſame manner the reſiduum of Scarborough water, and the ſucceſs was the ſame.

In theſe trials, it was not poſſible to approach ſo near as to feel whether the flame ſmelled of ſulphur or not, only it was more white, and going more on the colour of the flame of nitre in detonation. For underſtanding what reaſon there might be for ſuch appearance, I reflected on what I had formerly obſerved, on a parcel of the reſiduum of Scarborough water, that in two or three days, by attraction of a moiſt air, it had augmented conſiderably, both in bulk and weight: Alſo conſidering a vulgar expedient to make ſmoaking coals burn clear, by throwing

ſalt

ſalt upon them, I thought that the ſalt's impregnating the waters as above, might in great meaſure procure the difference. To determine this, I caſt equal parts of ſulphur and ſea ſalt in powder into a red-hot crucible, whence immediately aroſe a white flame, going on the blue towards the end; but, at the neareſt diſtance I could approach my noſe, I could perceive little or nothing of the ſulphureous ſteam; neither were others who ſtood by ſenſible of it. The flame ending, what remained was a grey acrid concrete. Nitre and ſulphur, mixed alſo in equal quantities, and thrown into the crucible, gave much the ſame flame, with a ſibilus, but no ſulphureous ſteam perceptible by me, nor others ſtanding by. After ſome time flaming, the maſs went into fuſion as a roſin at the bottom of the crucible.

Hereby appears that the ſalts attract the ſmoak of the ſulphur, imbibing it by the ſame mechaniſm as one of them attracts that of coal, and both of them the moiſture of the air; and ſo I think the difference of flames, as above ſtated, may be underſtood.

For finding whether there might be any thing regulin or metallic in either of theſe waters, I mixed both reſiduums with one half tartar and as much nitre. I tried firſt that of Scarborough water, the quantity being about two drachms, which was kept ten minutes on the ſtrongeſt furnace-fire in a crucible, with a cover of its own kind. After cooling, it was eaſily ſhaked out, retaining the form of the bottom of the crucible, and being white as before expoſed to the fire, only interſperſed with

ſome

some blackish particles, and on the upper surface appearing porous, and the whole resembling a spongy concrete of salt and chalky earth, the taste acrid, the weight exceeding but very little the weight of the tartar mixed with it.

Before trying the residuum of our water in the same manner, it came into my thought what appearance it might make when thrown into a red-hot crucible before being put into the fire. When this was done, it began to boil immediately with sparkling, and then cast up a thick cloud of smoak with a high bituminous smell. I regreted I had not thought on doing so by the Scarborough residuum, and the rather that I had not more of it for trial; yet what is said may in a great manner supply this omission. The residuum of our water appearing in fusion on the fire, and, after it cooled, adhering so close to the crucible, that it could not be got out but in parcels by an iron bodkin; it looked like a hardened dark rosin, and had an acrid taste: Which appearances, compared with that above of the fusion of sulphur and nitre into the same manner of substance, is, I think, a good addition to the proof of our well's sulphur going more on the nitrous kind, as the appearance of the Scarborough residuum, compared with what remained after flaming of sulphur and sea-salt, may prove, that its sulphur goes more on this salt, and that both are lodged in a calcarious earth as a substratum, wherewith, as by after-trials may appear, they are both so combined, especially the sulphureous part, that the violence of fire

 cannot

cannot disengage them, so that the sulphur does not always discover itself.

The decoctions of both waters, when farther carried on, may give further light into the above comparison. I boiled an English quart of each to about two ounces, and observing the progress, I found our water become still of a deeper colour, till, about six eighths being boiled in, it became like to a strong decoction of guajac wood, in the smell, taste, and colour; that of Scarborough, when boiled that far, resembling rather the colour of whey, and having the taste more briny, in respect of the resinous and mellowy taste of the other.

I attempted next to find if any of the substances usually employed in trying mineral waters, when mixed with the decoctions of these waters, and, by comparing their effects with those observed upon their mixture with the fresh waters as above, would, by the difference, discover any thing further concerning the nature of the salts in the waters, particularly, whether and how far they might be accounted volatile or fixed; whereof I could discover nothing that could be relied on in our water, kept, some of it a month, some two months, in cask or in bottles, shut or open, simply corked, or with the corks laid over with pitch; as neither could I perceive any difference of smell but what was common to any spring-water, when kept a considerable time.

To extend this comparative trial a little farther, I took in two other substances than were formerly employed, to wit, syrup of violets and

and tincture of turnsole, for which last, after Monf. Tournefort's way, I made use of blue paper.

1. *Syr. viol.* turned both our water and that of Scarborough into green.

2. Blue paper dipped in our water changeth not, only the blue appeareth more intense in our water. No change at all when dipt in Scarborough water.

3. A decoction of our water changeth not by *syr. viol.* neither does the blue paper change its colour thereby. But,

4. Scarborough water evaporated to one half, becomes, by *syr. viol.* conspicuously green. Blue paper dipped in the decoction changeth not colour.

5. Solution of sublimate corrosive on decoction of our water, at first makes no change; but, some time after, troubles it into a whiter colour, then precipitates a dusky residuum.

6. Solution of *sal. tart.* on the same decoction, gives presently a precipitate equable as a cloud.

7. Solution of sublimate corrosive changeth the decoction of Scarborough water gradually into a troubled dusky grey.

8. Solution of *sal. tart.* on the same decoction, makes no notable change.

9. Infusion of galls changeth not the decoction of our water, but presently makes a coagulum in decoction of Scarborough water, falling downwards from the top.

By the first of these trials it appears, that the salts of both waters, in their native condition, goes most on the alkaline, as it appears by Monf.

Monf. Tournefort's trials, the natural falt of the earth alfo does, whatever may be generally faid of its acid nature. It feems, by the third and fourth compared, that the falt in our water is more of the volatile fort, agreeing hereby more with nitre. The fifth alfo may feem to confirm the fame; but the feventh makes this more ambiguous, and the fixth renders it yet more doubtful; and the fixth, compared with the eighth, might rather feem to give the volatility to the falt of Scarborough-water; but the ninth to reftore the fame ftill to our water. In fuch counter appearances, I tried if any thing more certain could be made out, by affufing a little common water on the remains of the refidua of both waters left in the crucibles, after trying their inflammability, as above. On thefe infufions, I made over again the fame trials; but appearances were fo little confiftent to thefe above, or to one another compared, that I found little elfe but the folly of thinking, by fuch chemical attempts, to reach nature further than conjecture reacheth; and that, as I faid before, all we can pretend to by fuch trials, is to find out by analogy the congruity or incongruity of one thing we know, with another we feek after, without pretending hereby to come at the intimate principles of things, which we have hitherto in vain been hunting after by fuch means. All that we may account certain is, that nature affords nothing pure alkali or pure acid: Or, in other terms, whatever it be of any product of nature, which is proved of the one or the other nature by one trial, may be proved alfo of the contrary nature

ture by ſome other different trial. This Monſ. Tournefort hath proved by multiplicity of experiments on the natural ſalt of the earth, on the natrum of the Levant, on ſea and all foſſil ſalts. In fine, the violence of the fire cannot quite break this make of natural bodies, ſo as to deprive it quite of its native texture, as he hath alſo proved by his trials on lime-water, that the acid hath ſtill a being in burnt lime-ſtone. And I had been tempted to ſuſpect, that all my trials of analogy failed of proving the ſulphureous quality of our water, by ſeveral of the attempts to burn the reſidua failing, when they were expoſed to the greateſt violence of fire in a crucible, had I not ſeen the flame of the ſame reſidua in the furnace before. But what more myſtery is in this, than in ſteel waters failing in the trials by being expoſed to the leaſt evaporation, either when open to the ſun, or even in the common air, without any ſenſible influence of the ſun's heat, and yet nothing properly volatile is to be found in iron, or its vitriol? Such things put the minds of men to a ſort of nonplus. Yet ſtill we muſt take them as we find them, and be determined by nature, and not by our reaſonings.

I reſolved next to have a greater quantity of our water, for evaporation to a reſiduum, and to prepare it better for diſſolution of its compounding parts by putrefaction. For this purpoſe, I put twelve Engliſh quarts into a ſmall caſk, which I ſhut up cloſe, with the mouth pitched round, and placed it in a cellar during five weeks; and put about ſix quarts more in

 glaſs-

glaſs-bottles, ſome ſimply corked, others, alſo pitched, others open, ſome expoſed to the morning ſun, others in a cellar. I told before, that, by my ſmell, taſte, or trials, I could find very little alteration in our water, other than the taſte and ſmell the ſimple element has in ſuch caſe. Theſe eighteen quarts I put on the fire to boil and evaporate in two braſs pans, one ſmaller, the other larger; the ſmaller was open for moſt part during the boiling, the larger was covered from beginning to end. The quantities in each I did not mark. I had of ſediment of the leſſer, gr. xxxiv. of a grey duſky powder, as in evaporations formerly. The reſidence of the larger I obſerved coming to the conſiſtence of bitumen, and of a black colour, the decoction accordingly ſmelling and taſting high of the ſame kind. I poured off a little remaining water of the decoction, and, dropping on it ſome guts of *ol. tart.* I found ſtill the cloud formed, and ſtanding all the day without any curdling precipitate. I found the bitumen, after the water was evaporated before a fire in a tea-plate, weighed gr CLXX; and, expoſing it again to the fire till it became a roſin, I had drach. ii. gr. xi. Of this I threw ſeveral parcels into a red-hot crucible, placed in an ordinary fire of no intenſe heat, and every parcel quickly flamed about two minutes, then almoſt vaniſhed. I next tried the powder I had as reſiduum of the ſmaller decoction; but it did not flame, but jetted out ſparkles of flame, huzzing as wet gun-powder does, when touched with fire. I can attribute this variety to nothing elſe but the

the larger pan's being covered during the decoction, whence I had the bitumen; and, upon ſight of it, compared with its taſte and ſmell, concluded that I ſhould have my reſiduum more inflammable, or eaſier to take flame than formerly. Of ſuch conſequence it may be, more than we ordinarily think on, to have our decoctions, whatever they be, more or leſs circulated.

Upon the whole, it may appear, that analogy right ſtated, and juſtly regarded, is the ſureſt way by which our reaſon is capable to lead us into the nature of things; and that experiment, juſtly ſtated and managed, will always anſwer to ſuch analogy. And hereby is confirmed the likeneſs of our water to that of Scarborough, as to their ſpecific contents, though in proportions different from each other; the ſalts abounding more in that of Scarborough, the ſulphur appearing to abound more in ours, the different bulk or weight of the whole conſidered, which is much greater in Scarborough water than in ours. And yet it will next appear, that equal quantities drunk, the phyſical operation of ours hath proved not inferior. Analogy hath taught me the preſence of theſe ſalts; Dr Short and Dr Shaw have found them by direct trial. Thus does the fact confirm the analogy, as I ſtated it betwixt the experiments on different ſubſtances, and theſe on our well, and of conſequence betwixt our well and that of Scarborough. The evidence appears equally concerning the nitre and ſea-ſalt in both. Concerning the reſpective quantities whereof in each water, I had occaſion

to

to make an obſervation pretty remarkable, but whereon at firſt I was little thinking.

Having remarked, that a parcel of the Scarborough reſiduum, two or three days after evaporating its moiſture, appeared conſiderably more bulky, I weighed and found it drach. ii. very near, then put it in a tea-plate before a warm fire two hours, and found the weight diminiſh near one third, which anſwered to my ſuſpicion, that its comparative bulk was from the moiſt air it had attracted, the weather having been ſomewhat foggy. I had obſerved no ſuch appearance in any reſiduum I had got of our well: But, to be more ſure, I tried in the very ſame manner, while the weather was moiſt, gr. xviii. I had remaining of the reſiduum of laſt decoction. When the plate was taken hot from the fire, I found the weight diminiſhed not above gr. i. ſem. This with the comparative colours I always obſerved of the reſidua of each, that of Scarborough water going conſtantly on the grey white, and that of our well on the duſky brown, leſs or more intenſe, proves ſtill the prevalence of the ſalts in Scarborough water, if not of the calcarious earth, and of the ſulphur in ours. To which, finally, may be added, that, having brought my laſt decoction of our water to about a gill, I dropped thereon ſome *ol. tart.* and it formed a thick cloud of an equable ſurface and conſiſtence all over, and ſtanding firm ſeveral days, without ſeparating till broken on deſign, which had not occurred to me before in any former trials; and it appears above,

bove, that decoction of Scarborough to one half, gave no ſuch appearance.

*P. S.* As I could not, by evaporation, bring our water at any time to caſt out any cryſtals, I deſigned, at laſt decoction, to have tried the ſalts adhering to the ſides of the pan; but, not finding how to determine any thing thereby, and fearing to tranſgreſs my bounds, I cauſed them to be waſhed down into the decoction.

## IX. *Medical Qualities of* Montroſe *Well, with ſome inſtances thereof;* by the ſame.

THE water of this well is of an obſcure mineral taſte, which cannot well be deſcribed. It is lighter on the ſtomach, and eaſier to digeſt than the fineſt ordinary ſpring water, or any other of our mineral ſprings; ſo that delicate conſtitutions bear it eaſily, and are improved by it in their appetite and digeſtion. A man about ſeventy years of age, and of a decrepit habit, bore eaſily, even when he began firſt to drink it, two Engliſh quarts in the morning, and near as much after noon, with alleviation of a cough he laboured under, and told me, though he was of a low ſtation of life, he could never formerly bear a draught of any manner of water.

It clears the ſpirits of heavineſs, and gives life to the inward powers, even of theſe of the more ſober and low diet. The ſimple element, whether cold or hot, infuſed on tea, by diluting

ting, and in either way, by its ſtimulus, waſhing off the indigeſtion, which clogs the natural faculties, in thoſe of high or full diet, ſerves ſo far well to this purpoſe. But thoſe of ſlender conſtitutions, and who live on low diet, have found this effect by a few cups of this water. Some of the more ſober and delicate females have alſo experienced this. And a gentleman uſing the vegetable diet, and of the animal nothing above milk or eggs, and the ſimple element for drink, of a thin habit of body, and with lively ſpirits, found himſelf generally yet more chearful, and his appetite and digeſtion improved, by drinking only about an Engliſh pint.

It is univerſally diuretic, people accounting generally that they paſs more that way than they drink.

Being drank from two to three pints Engliſh, it generally purgeth by ſtool three or four times, the half of that quantity ſucceeding in ſome. By this quantity a gentleman, from beginning to drink of it, was ſet a purging three or four days together, during which he was obliged to refrain its uſe, and afterwards to diminiſh the quantity, till he was more accuſtomed to it. The more plentifully one feeds, it generally operates more, as hath been obſerved concerning other purgative ſpaws. In ſome, the ſame degree of operation hath continued during the uſe of the water: But generally, as people are more accuſtomed to it, they are obliged to intermit at times its uſe, or to augment the quantity.

Soon after its medical virtues were firſt obſerved, it performed a cure, ſcarce inferior, I be-

believe, to moſt, or any recorded in obſervations of medicine. A girl during nine years, almoſt from a child, afflicted with ſtrangury, ſtoppage of urine, and, for moſt part, of the evacuations of her belly alſo; her urine being conſtantly as a thick gleet, and reſembling the mucilage of oat-meal; ſhe having, during moſt of the above time, ſcarce any intermiſſion of ſymptoms, whereby her fleſh became waſted, and her ſtrength ſo much impaired, that ſhe went on crutches, or otherwiſe ſupported. At length the paroxyſms taking her conſtantly ſo ſoon as aſleep, ſhe became as one inſenſible, talking, and anſwering things ſpoken to her out of purpoſe. She being then ſupported to the well, began immediately, being under no direction, to drink freely, and it paſſed by ſtool and urine as freely, with immediate eaſe. This encouraged her to too much freedom, till, over-charging nature, ſhe was threatned with a ſuffocation, and hereby was taught to uſe it more moderately. I gave her ſome directions for drinking it afterwards, and, the evacuations ſtill ſucceeding, ſhe not only was cured of all diſeaſe in a few weeks, but alſo became of a clean and natural full habit of body; when ſhe declared, that, for nine years bygone, ſhe had not enjoyed ſo much freedom from trouble, taking all in one, as ſhe had ſince ſhe began to drink the water.

In general, as a good many, in various ſymptoms of gravel and nephritic pains, have come to it for relief, ſo I have found none,

who have not been more or lefs, and moft of them remarkably, relieved by it.

A gentleman, during a good many years, afflicted with paroxyfms of the ftone, rarely under twice a year, and fometimes they feized him more frequently, each paroxyfm only going off after paffing ftones of various bignefs, by drinking in the morning the above quantity, or a little more of this water, it going off plentifully by urine, and by moving the belly a little, became frefher in all his natural and animal faculties, and has continued almoft a year free of all nephritic paroxyfms, except fome fmall threatnings. He then left this place, and I know not how he has been fince. One circumftance in his cafe was moft remarkable, that, during the ufe of the water, he paffed urine always without any ftimulus or inclination to pafs any more, which had conftantly affected him before, fince the firft attack of the difeafe.

Another gentleman, during feveral years under grievous fymptoms of ftrangury, and fometimes paffing gravel, feveral means of relief, and particularly the Aberbrothock water, being ufed to no purpofe; at laft there was ftrong fufpicion of an ulcer in the neck of the bladder where he ailed moft, from a purulent matter paffed with his urine. After all hopes of life were almoft gone, he began infenfibly to become eafier of the fymptoms; but the difeafe ftill remained, and particularly his urine paffed with difficulty, and he had violent pain from the ftrangury always after paffing it. From beginning to drink a pint

or

or little more of the Montrofe water in a morning, and drinking it warm infufed on tea after noon, his belly being opened a little after his morning draught, he began to pafs his urine every day with more freedom: The pain diminifhed alfo daily; which good effects he had not found before from any water, fimple or medical, neither from any other liquor he drank.

As it generally relieves in all fcorbutic fymptoms, and foulnefs of the blood, fo hath it perfectly cured feveral inveterate fcurvies in the younger and older, above any effects of medical waters I have hitherto obferved.

A gentleman, otherwife of a ftrong conftitution of bowels, and of a vigorous body, but labouring a good many years under the fouleft eruptions on his fkin, almoft coming to a degree of leprofy, by drinking from three to four Englifh quarts a-day, which gave him three or four ftools, and dipping his fhirt among the water for a bath over night, was, from the worft degree of eruption he had been liable to, brought almoft incredibly to a perfect cure, in lefs than three weeks. The eruptions that were thick, inflammatory, and fending out a fanies, as I obferved them in the beginning, going all away, and his fkin becoming all over foft and fmooth, as I alfo faw; and he has continued found and in perfect health now above a year, fometimes drinking the water at about fix miles diftance.

A woman of about fixty years of age, of a valetudinary ftate of health, and particularly much afflicted, during feveral years, with

 fcorbutic

ſcorbutic runnings over ſeveral parts of her body, her head alſo being greatly affected in the ſame way; diet-drinks of ſeveral ſorts given and repeated proving of no uſe, by about a month's uſe of about three Engliſh pints of this water a-day, whereby ſhe purged three or four times daily, was entirely cured, and remains ſo now more than a year.

A girl ſeven or eight years of age, labouring, almoſt from her birth, under a dry itching ſcurf over her ſkin, and ſeveral meaſures being taken for her cure in vain, by drinking three weeks daily, about two thirds of a pint of water a-day, whereby ſhe had the ordinary ſtools, and bathing therein twice a-week, was made entirely clean. And the diſeaſe having threatened ſometimes to return by unguarded treatment, ſhe hath been always cured by the ſame means, and is now perfectly ſound.

By other inſtances of cures made, or relief given by the uſe of our water, it would ſeem to be of pretty good ſervice in moſt chronical diſeaſes, and to reach from the firſt organs of digeſtion, to all that affect the blood. In winds and flatulency ſtifled in the bowels, it hath proved of good uſe, breaking the wind with ſuch force, that people, the females eſpecially, have been afraid to drink it in public; and ſome who have been long afflicted this way have been perfectly cured by it. Others have found benefit by this water, who could not bear drinking the ſimple element cold. So hath it alſo made compleat cures by ſtool and urine in ſpaſmodic colics, called by the

vulgar

vulgar windy gravel. To this claſs we may even likewiſe bring the caſe of a woman labouring under racking pains of her breaſt and head, who, having paſſed turbid urine in large quantity, by drinking it, was cured without any purging.

It has alſo been beneficial in diſordered ſecretions from the blood, and where the liquors have been in a ſtagnating way. Among the reſt, the caſe of a young man was remarkable, who, after a tertian ague of three months continuance, having ſwelled all over the belly, arms, and head, was cured, by drinking, without allowance, what he could of the water, from ſun-riſing in the beginning of Auguſt, to about nine o'clock every day of two weeks. But the caſe following is more ſingular.

A woman falling into blood ſpitting, (ſuppoſed to have been occaſioned by a ſtrain ſome time before), which continued about a year and a half, recurring leſs or more every three, or at moſt four days, and frequently ſooner, notwithſtanding her menſtruating regularly, from the time ſhe began to drink this water, which ſhe continued for two weeks or more, ſhe ſpit none, neither did any bad ſymptom appear, although the water operated every day, both by vomiting and ſtool. She began by drinking two quarts. On hearing which, to prevent ſuch operation, I ordered her to take not above the half; notwithſtanding this, it continued to have the ſame effect; and I am juſt now told, that ſhe continueth well.

In rheums and ſtrains alſo I have found it of good uſe, by way of cold pump, where the perſons could not bear ſuch uſe of ordinary cold water.

A gentlewoman of a thin habit, and advanced age, being afflicted with a rheum of a pretty long continuance in her arm, had the pain allayed, and the rheum as good as quite cured, by the water uſed in that way: But the rheum ſome time after attacking the other arm, ſhe was afraid to admit of the cure.

A gentlewoman having ſtrained her foot, whereby, notwithſtanding variety of cures, ſhe was confined during moſt of a year, could not move her toes, nor put her ſole flat and even to the ground, the fleſh of her leg being alſo remarkably waſted, her ſkin became diſcoloured, rough, and as it were dried; not having been able to bear the application of ordinary cold water to the recent ſtrain, by bathing and pumping this water on her foot and leg, about two weeks, ſhe began to move her toes, then all her foot freely, and reſted it level on the ground; her leg alſo recovering its native colour and ſoftneſs, and becoming plumper; ſo that ſhe began to walk about a little, ſupported by a crutch, and ſometimes only by a ſtaff, when, by an unhappy accident, falling on the ſame leg again, and her foot folding as it were under her, all her ills returned to almoſt a worſe condition than in the beginning: And ſhe, after not finding the immediate benefit of the water, was impatient to lay it aſide, that ſhe might try other cures, which hitherto have had no ſucceſs.

A poor man, at once paralytic of his limbs, and wrong in his judgment for several years, as I am well informed, coming to our well on crutches, without which he could not stir; in a few weeks, by drinking what he could get of the water, and bathing his limbs in it when he found none to hinder him, was so well cured, notwithstanding his being seen drunk at times, that I saw him walking about sound, carrying his crutches.

I could have given other instances of cures done by this water, had not my avocation in the country, and business when in town, detained me from so closely attending the well, in this, as in the former season. But, perhaps, these are enough for the bounds of this paper.

The disadvantages of our well are, its being a little too much exposed, being close on an open road, without convenient covering for walking about: This could not well be helped, without some apartment being erected for the better sort, females especially. Then the spring is but small, giving only two English quarts in a minute, too little for affording sufficient water for the demands of bathing the body, but more especially for the drinking of any confluence of people. To remove both defects, I will endeavour, next spring, to have a stone-bason, with a cover, made, as a reservoir for the water in the ground below the well.

X. *An Essay on the Method of preparing and preserving the Parts of animal Bodies for Anatomical Uses; by* ALEXANDER MONRO, *Professor of Anatomy in the University of* Edinburgh, *and* F. R. S.

THE moſt artful management of the knife alone cannot diſcover the texture of animals ſufficiently; anatomiſts are therefore obliged to employ ſeveral other arts, which often require a conſiderable time in their execution, and frequently fail through ſome unfitneſs in the ſubject, or by a very ſmall omiſſion or accident: And, when they ſucceed in any attempt of this kind, they endeavour to preſerve the parts thus prepared, that they may always be in readineſs to ſupply what cannot be demonſtrated on the recent ſubject. While each anatomiſt is obliged to his own induſtry only for the diſcovery of theſe arts of preparing and preſerving, it is probable that frequent diſappointments, and the deſpondency theſe will create, may diſcourage many from purſuing the ſtudy of anatomy, who might become the greateſt improvers in it, if theſe difficulties did not lie in their way. With a view to remove them in ſome meaſure, I ſhall lay before you in this paper ſuch arts of preparing and preſerving the parts of animal bodies as I have found ſucceſsful, which may ſerve as a foundation on which others more induſtrious or ingenious may build.

The principal preparation that BONES require is to make them white; for which directions

rections are given at considerable length by Pauli * and Lyserus †, who also teaches the method of putting the bones of adults together. What I have found most successful for whitening the bones of young creatures, is macerating them long in cold water, which ought to be changed frequently; the bones being laid out each time to dry a little in the sun. If they lie too long in the water, even those of adults will dissolve in their more spongy parts, and the younger ones lose all their epiphyses. If they are allowed to dry before the blood in their vessels is melted down by the maceration, it scarce can afterwards be separated from them, or they will never become white. The marrow of young bones being much less oily than in adults, they generally can be made much whiter, and do not readily turn yellow by keeping. The bones of fœtuses require to be taken frequently out of the water; and the periosteum must not be separated where the epiphyses are joined, otherwise it is scarce possible to prevent the loss of these additional pieces.

The method of burning and exposing the bones of adults long to the weather, for unravelling their texture, is so generally known, I need scarce mention it.

CARTILAGES are made pellucid in the same way that bones are whitened, and they must afterwards be brought to their natural shape and situation, in which they are to be kept by strings,

* Act. Hafn. vol. 2. § 18.
† Cult anat. lib 5.

ſtrings, weights, pins, and ſuch other contrivances, if they are to be preſerved dry.

The MUSCLES that have no large cavity, are to be laid and ſecured in the poſture they are deſigned to be preſerved in, and they are preſſed with the fingers into a natural ſhape while they are drying.

You have already publiſhed all I know of the method of injecting the VESSELS. [See Art. IX. of vol. I.] That they may appear better, it is neceſſary to macerate in cold water all injected parts that are coloured with blood, till the blood is extracted; after which the water is to be preſſed well out; and, even when the preparation is to be preſerved wet, it is of ſervice to let it dry a little in the air, before it is put into the embalming liquor. But, before the very minute extremities of injected veſſels can be demonſtrated, there is a farther art requiſite, which is the ſame that has been employed by ſeveral of late for unravelling the texture of leaves and fruits, of which Severinus *, near a century ago, ſays, "Verum "me ineptum, quid præterieram opontii folii re- "ſolutionem artificioſam: Parabitur hæc porro "ſimplici tabefactu per affuſam uberem aquam "tamdiu complexuram, dum fibris lignea duritie "reſtibilibus, omnis exſolvatur carnea portio." And Ruyſch † at laſt acknowledged his method of preparing the ſuccous veſſels of fruits, and of the brain, &c. to be the ſame. Put therefore the injected brain, lungs, liver, ſpleen,

* Th. Bartholin epiſt. med. cent. 1. epiſt. 65.

† Adverſ, anat. dec, 3. § 2.

ſpleen, placenta, or any other part of a tender texture into water, allow it to remain there till its involving membrane is raiſed by the water inſinuated into the cellular membrane, which connects it to the parts below; then ſeparate the membrane, and afterwards keep the part among the water, till the fibres, connecting the ſmall veſſels, are diſſolved: This is known by ſhaking frequently the part among the water, which waſhes off the corrupted particles, and at laſt the ſuccous veſſels are ſeen diſtinct and floating in the water; then the preparation is taken out, and, by gently preſſing, the water is ſqueezed from it, the remainder of it being waſhed away with ſome of the perſerving liquor, into which it is immediately put, and, by a little twirle of the thread or hair by which it is ſuſpended, the preparation is expanded, and the ſmall veſſels are ſeparated.

I never could divide nerves into their very ſmall filaments, after they had got their firm coat from the *dura mater*; but, before they are involved in that membrane, they are eaſily divided. Thoſe that conſtitute the *cauda equina* are fitteſt for this purpoſe; for they are long, and their fibres adhere by very weak fine membranes. One of theſe chords being cut through, where it comes off from the *medulla ſpinalis*, and where it is about to enter the *dura mater*, one end is ſecured with a hair, by which it is ſuſpended in a baſon of water; and, after macerating ſome time there, it is raiſed to the ſide of the baſon, upon which it is laid, while one hand holding the hair, with the

the other a very ſmall needle fixed in a handle, is ſlightly ſtriped along the nerve. Let this operation be continued, till, upon twirling the nerve among the water, it is expanded into a fine web of very ſmall fibres, when it is put into the embalming liquor. If the blood-veſſels were previouſly injected, the hair muſt be tied on the end of the nerve neareſt to the *dura mater*, that the trunk of the nerve and artery may appear together: When a piece of the *cauda equina* is thus prepared, it ſhews very prettily; for each filament almoſt of the nerve appears with injected veſſels upon it.

When a fine ſingle MEMBRANE, ſuch as the pleura or peritoneum, is to be perſerved for demonſtrating its arteries after an injection, as much of the cellular membrane, by which it is connected to the neighbouring parts, is to be ſaved in diſſecting it off as can be done without ſpoiling the tranſparency of the membrane; for, when the cellular ſubſtance is wholly ſeparated, very few continued ramifications of veſſels can be ſeen, a great number of exceeding ſhort extremities only appearing, which require a microſcope to view them diſtinctly. When there is little fat lodged in the cells, the membranes of the cellular ſubſtance may be left without a poſſibility of obſerving them: When they are filled with fat, it muſt be preſſed out as much as poſſible, after being well macerated. Even when membranes are to be preſerved in a liquor, they are more eaſily kept extended in it, and their veſſels are better ſeen, if they are previouſly dried. In doing

ing this, they muſt be extended with pins or threads on a finely poliſhed clean board, or it is rather better to keep them on the ſtretch while they are raiſed up from it, that they may bear no print of the board. After membranes are dried, their doubled edges, or other inequalities, are to be cut off with a pair of ſciſſars.

Ruyſch * deſcribes the manner of ſeparating the cuticula and *corpus reticulare* from the ſkin, by ſtretching theſe common teguments, well freed from the fat, on a board, with the cuticula outmoſt; and then dipping all into boiling water, which looſens the cuticula and *corpus reticulare* ſo from the ſkin, that they are eaſily ſeparated with a blunt knife, or the thin ivory handle of a ſcalpel; then with the ſame inſtrument he ſeparates the *corpus reticulare* from the cuticula, leaving them connected to each other, and to the ſkin in ſome part. After this they may be either dried or put into the embalming liquor. When either the *corpus reticulare* is not made firm enough by the hot water, or happens to be very thin, it is difficult to ſeparate any conſiderable piece of it entire from the cuticula.

A chirotheca or podotheca, a glove or a ſhoe of the ſcarf-ſkin, with the nails adhering to it, is brought off with very little trouble after the cuticula looſens from the parts below by the putrefaction, which keeping a ſubject long brings on. This method anſwers the deſign better than forcing off the ſcarf-ſkin with boiling

* Adv. anat. Dec. 3. § 2.

boiling water, which makes the cuticula tender.

The cellular membrane under the ſkin cannot be preſerved diſtended with air, except where there is little or no fat contained in it. One of the beſt parts for making a preparation of this kind is the ſcrotum, where what is commonly called *muſculus dartos* may, by blowing into it, be changed entirely into fine membranous cellules; and Carolus Stephanus * very well obſerved, that the cellular ſubſtance any where elſe under the ſkin puts on a muſcular appearance when the fat is waſted. Will not theſe obſervations ſerve as ſome apology for former anatomiſts who reckoned a *tunica carnoſa* among the common teguments of the body? May we not hence ſee one reaſon of old emaciated people having ſo many wrinkles in their ſkin?

That the DURA MATER with all its proceſſes may be preſerved in a natural ſituation, it is neceſſary to ſaw the cranium from near the root of the noſe to the middle of the *os occipitis*, by a perpendicular ſection at half an inch diſtance from the ſagittal ſuture, and then by an horizontal ſection, terminating at the extremities of the former perpendicular ſection, to take off a conſiderable ſhare of the ſide of the cranium. After which the *dura mater* being cut by an inciſion in form of a T, the brain and crebellum are taken out, and the head is put amongſt a liquor to preſerve it; or the bones are made clean and

* De diſſect. part. corp. human. lib. 2. cap. 2.

and exposed to the air to dry, taking care to keep the cut parts of the *dura mater* stretched out with pins, hooks, or threads. If the head of a foetus, or of a very young child, is thus to be dried, the ligamentous membranes between the bones must be kept extended by sticks cut of such a length, as, when put into the cavity of the cranium, their extremities may rest on the bones, and push them outwards.

The processes of the PIA MATER which are placed in the interstices of the convolutions of the brain, may be easily separated entire with the *pia mater*, when that membrane is preternaturally thickened by diseases, as it frequently is: And, even in a sound state, large pieces of it may be got with its processes, after macerating the brain in water. As soon as it is separated, and the water pressed from it, it ought to be immersed in the embalming liquor, where it is to be kept extended by threads or small branches of plants.

The BRAIN requires no preparation, except either for demonstrating its succous vessels, the method of doing which I have already mentioned; or for hardening it, which I shall afterwards speak of.

Before the coats, humours, and vessels of the EYE can be rightly prepared for preserving, so as to demonstrate all of them, it is necessary to coagulate the crystalline and vitreous humours, by immersing the eye for some time in a proper liquor; of which hereafter. After this, they will bear maceration in water,

for the ſeparation of the choroid and Ruyſch's coat.

The ſebaceous glands and ducts of the eye-lids appear much better after a ſubtile injection of the arteries, and the coagulation of their liquors, than in the recent body.

Dr Trew * has very juſtly obſerved, that, by macerating the EAR in water, the membrane which is continued from the epidermis of the ear to line the *meatus auditorius externus*, and to form the outer lamella of the *membrana tympani*, may be brought off entire in adults, as well as it is eaſily ſeparated in fœtuſes, or very young children. And indeed the *membrana tympani* appears to be no other than this cuticula, and the membrane that lines the tympanum, connected by a thin cellular ſubſtance, in which, as in all other ſuch parts of the body, the larger branches of the veſſels run.

The epithelia of the LIPS, as Ruyſch calls the cuticula covering the papillæ there, is to be taken off by macerating in water, which makes the ſurface appear better in the villous way, when the lips are afterwards put into a glaſs with the embalming liquor.

The villous ſubſtance of the TONGUE is very eaſily made quite red, with injection thrown in by the arteries, and a membrane analogous to a cuticula ſeparates by ſoaking in water; and, upon comparing the lips, tongue, œſophagus, ſtomach, and inteſtines, the ſtructure ſeems to be ſo far alike in all, as they are covered with this ſort of cuticula, which is connected

* Act. phyſico-medic. Acad, N. C, Vol, 2. obſ. 56.

nected to the muscular part by a cellular substance, in which the numerous nerves, vessels, and glands are lodged. This cellular substance either is formed into ridges and valves where it is thick and loose, or appears like a fine membrane where it is thin and stretched.

There are no organs in the body of which I find a greater difficulty to give the students of anatomy a good idea, than those employed in deglutition. In the recent body, they cannot see them all at once in the natural situation: They can scarce be held in a wet preparation, so as to shew them well enough. What has the best effect, is to demonstrate the grosser parts first in a dry preparation, which requires patience to execute right; for all the muscles belonging to these organs, that are fixed to any of the surrounding parts, must be all clean dissected, and then cut off from these surrounding parts; after which the tongue, *os hyoides*, *fauces*, *velum pendulum palati*, with the uvula, the larynx, pharynx, *aspera arteria*, and oesophagus, are taken out with the dissected muscles hanging at them, to be all secured in their natural situation with small pieces of thin boards, and threads fastened to hooks. A cork is then put into the lower part of the trachea, round which and the oesophagus a strong thread is tied firmly, and quick-silver is poured by the fauces, or by the passage which formerly led to the nostrils, till the oesophagus, trachea, larynx, and pharynx, are filled; in which condition it is allowed to hang till the parts are pretty firm, but not near fully dry, when the quick-silver is poured out, and the

the parts overſtretched by its weight, ſuch as the glottis and ſpace between the tongue and larynx, are, by preſſing and ſqueezing with the fingers, to be brought near to the natural ſituation; and others that ſhrivel too much, as, for inſtance, the uvula and epiglottis, are kept near to the natural form, by drawing and preſſing them from time to time till they are fully dried.

The hollow VISCERA of the thorax and abdomen are not only to have their veſſels demonſtrated in the ways mentioned formerly, when ſpeaking of the blood-veſſels; but, when they are to be kept dry, they require a particular preparation for preſerving their form, and for ſhewing their interior ſtructure, which is to fill them with ſome proper ſubſtance. The properties I think this ſhould have are, to be able to reſiſt the ſhrivelling contraction of their fibres, to fill them all equally, and to leave them clean if it ſhould be taken out. For theſe reaſons, cotton, wool, ſand, or ſuch like, are improper; all that I uſe is air, quick-ſilver, or melted wax.

Wax is only to be employed where there is no farther deſign than to view the exterior ſurface; for which purpoſe it may be thrown into any of the hollow bowels; but, in all other caſes air or quick-ſilver muſt be uſed.

Where air will anſwer the deſign, it is preferable to the quick-ſilver, for it ſtretches every where equally; whereas mercury preſſes moſt on the depending parts. Air dries the bowels in the twentieth part of the time that quick-ſilver does, and it leaves no colouring or gilding

gilding on them; which the mercury always does. On the other hand, air does not ftretch fome parts fufficiently, cannot be retained, and infenfibly efcapes fo from others, as to allow them to collapfe in drying; which inconveniencies the quick-filver is not fo fubject to.

From what has been faid, it is evident, that air is required, or is much preferable to quick-filver, for making dry preparations of the œfophagus, ftomach, guts, *vefica fellea*, with the biliary ducts, and bladder of urine, with the ureters. And it is as plain, on the other fide, that the pericardium and uterus can only be kept diftended in their natural form with mercury.——The heart with its blood-veffels, and the pelvis of the kidney with the ureter, generally have fome fmall paffages for the air to efcape at, and it fcarce can refift the fhrivelling contraction of their fibres; wherefore mercury is preferable in drying them. The *corpora cavernofa* of the penis, and the *veficulæ feminales*, retain both air and mercury; but this laft leaves a gilding in the *corpora cavernofa*, which hinders fuch a clear view, as could be wifhed, of their veffels and texture; and there is a difficulty to fill the *veficulæ feminales* with it, for the orifices at the *caput gallinaginis* will not admit it from the urethra; and, when it is poured in at the *vas deferens*, the moifture of this narrow pipe is liable to ftop it; and, after you have got it to run in this canal, its weight forces open the orifice into the urethra of the fmall duct common to the *vas deferens* and *veficula feminalis*, fo that it will not mount into the veficula till the urethra is filled: Where-

as the contraction of that orifice resists a small blast of air, which readily regurgitates into the vesicula. For which reasons, air is prefe-rable for the *corpora cavernosa* and *vesiculæ seminales*.——It is seldom we meet with subjects whose lungs or spleen will retain air, and the glans of the penis is very liable to allow it to escape; wherefore, generally, we are obliged to make use of quick-silver for these parts; which, however, does considerable prejudice in all of them, but especially in the lungs and glans, whose cellules are smaller than those of the spleen.

Being determined, by the foregoing rules, which of the two liquors to use, we press out all the blood or other liquors contained in the bowels we design to fill, and then tie all the passages from them, except the one by which we are to introduce the distending liquor; and, in case we discover any others in the time of filling them, at which the air or quick-silver escapes, we tie it likewise.

The passage by which the liquors should be introduced, is to be chosen such as soonest allows them to pass every where into the cavity to be filled, and that is most easily secured afterwards. What was said of the organs of deglutition, will readily lead one to know how to distend the pericardium and uterus. The place for blowing up the alimentary tube, *vesica fellea*, and *urinaria*, is known to every body. Liquors are poured into the heart and large arteries through the superior *vena cava*, and any branch of the pulmonary veins. The *trachea arteria* receives what the lungs are to

to be distended with. The kidney is to be filled by the ureter. The veins of the spleen, *capsula atrabilaris*, and *corpora cavernosa penis*, are to convey the substance with which they are distended.

We must always make use of a pipe, when we are to blow up any part. The best sort of pipe for this use is what has a small extremity, with a notch round it, and a stop-cock a little higher in it. We introduce the small extremity into the proper canal which is tied upon it with a waxed thread, that is made to sink into the notch; and, as soon as the bowel is sufficiently distended, the stop-cock is turned, to prevent the air to get out. If any of it escapes, it is easily supplied by a new blast at the pipe, which is supported by some string or board to hinder it to press or draw the preparation while it is drying.——If the anatomist is provided with no other than a common blow-pipe, the canal by which the air is introduced must be tied tight upon it with a thread, which is drawn with a firm knot by an assistant, while the air is blown in. As soon as the bowel is sufficiently distended, a signal is given to the assistant for pulling the ends of the thread, while the pipe is drawn out of the canal, and immediately another knot being made, the preparation is suspended by this thread.

When quick-silver is employed, the passage by which it is poured must be higher than any other part of the preparation; and when that passage is narrow, a small glass funnel or pipe must be put into it. This pipe must be long, where

where the weight of a high column of mercury is wanted to make it run through small veſſels. If the preparation will allow, the paſſage by which the mercury entered is to be tied firmly, otherwiſe, before any mercury is poured in, that paſſage muſt be ſecured, ſo as to remain uppermoſt all the time the preparation is drying.——When a conſiderable quantity of mercury is poured into any part whoſe texture is tender, it is neceſſary not only to ſuſpend it with threads and hooks in the ſuperior part, but to ſupport it alſo by a ſmall net extended below it, upon a proper veſſel for receiving the quick-ſilver, if by any accident it ſhould run out.

The directions now given will ſerve for preparing the moſt part of the bowels; but the LUNGS and SPLEEN, whoſe membranes very difficultly retain either quick-ſilver or air, eſpecially this laſt, require more care. Theſe bowels muſt not be taken indifferently from any ſubject; but ſuch muſt be choſen as have the exterior membrane thick and ſtrong. After they have been diſtended as above directed, they ſhould be expoſed to the warm ſun, or near a fire to dry them ſoon, returning frequently to ſupply by a new blaſt what they loſe in very little time. As ſoon as their outer ſurface is dry, immerſe them into ſtrong turpentine varniſh, ſo as their whole ſurface may be covered, after which they will retain the air much better; continue to place them ſo that they may ſooneſt dry, taking care to rub on varniſh with a feather, where-ever it is wanting,

ing, and to blow in new air whenever they subside.

After the human spleen has been kept distended with quick-silver or air till it is dried, it seems to be wholly composed of cells communicating with each other, upon the sides of which the small branches of the artery are observed to spread in great numbers, if they have been previously injected, as you may see when you please in several such preparations now in my possession.

Upon cutting the lungs thus prepared, their vesicles appear far from being spheres, of any other figure whose transverse section is circular, for they evidently are polygons, generally irregular squares and pentagons. And indeed one might conclude *a priori*, that they are rather more so in a living creature; for, seeing the exterior membrane is of a firmer texture than the vesicles, and will not stretch out so far as they could be extended, they must push violently on each other; and therefore be pressed from a spherical form into as many sides and angles as there are contiguous vesicles; and the thorax of a living creature does not allow the lungs to be extended so far as their exterior membrane can be stretched, as is evident from their starting out at wounds of the thorax, or by blowing into the *trachea arteria*, after the sternum of a creature is taken away; and therefore their vesicles must be more compressed, and consequently their sides be straighter in respiration, than when they are distended after being taken out of the body. These considerations, and the obvious

obvious polygon form of the cells in the simple lungs of serpents, frogs, &c. makes me surprised that ever the vesicles of more complicated lungs should have been imagined to be spheres, or any other figure, whose transverse section is circular.

I come now to consider the manner of preserving preparations, which is either by exposing them to the air, till all their moisture is evaporated, and they become dry, rigid, and out of hazard of corrupting; or by immersing them in a proper liquor. Besides what has been already said concerning the manner of drying preparations, it is also necessary, especially when the parts are thick and bulky, and the weather is warm, to guard further against putrefaction, to hinder flies to lay their eggs upon them, which soon growing into maggots would destroy them, and to prevent insects, mice, or rats, to feed on them. All this may be done by soaking the preparation, some time before it is exposed to dry, in a solution of corrosive mercury in spirit of wine. The proportions I use, are two drachms of the corrosive to a pound of the spirit; and while it is drying, it is to be moistened frequently with the same liquor. By this method one can, without any of the dangers above mentioned, dry the dissected bodies of pretty large children in the midst of summer, which sooner answers the design, than the cold moisture of winter can.——After the preparation is dry, it is still liable to moulder away, become brittle, crack, and have an unequal surface;

face; wherefore it is necessary to defend all its surface with a thick varnish, with which it is to be covered over as often as the lustre of the former application wears off; and it must always be kept well from all dust and moisture. The varnish I always use is the common spirit one, which I buy from the painters *.

Dry preparations are very useful in several cases; but there are a great many where it is necessary the parts should be flexible, and nearer a natural state, than the shriveling and rigidity in this way of preparing can allow: The difficulty has hitherto been to find a liquor that would preserve them so near to a natural condition. Watery liquors do not prevent the putrefaction, and dissolve the hardest parts of the body. Acids prevent putrefaction, but dissolve the parts into a mucilage. Ardent spirits harden the parts, change their colour, and destroy the red colour of the injected vessels. Oil of turpentine, besides the faults of the ardent spirits, becomes thick and viscous. But, without dwelling longer on the faulty liquors, I shall inform you, That what I have always found to answer best, is a rectified ardent spirit, no matter whether from wine or malt, that is still limpid, without having any yellow tincture, to which a small quantity of a fossil acid spirit, such as that of vitriol, or nitre, is added; both of them resist putrefaction, and what might be looked on as faults, if each was considered single, is mended by the other.

When

* Ruysch recommends a varnish made of *gum. copal.* and *ol. spicæ.*

When these are mixed in a right proportion, the compound liquor changes neither the colour nor consistency of the parts, except where there are serous or mucous liquors, which it hardens much in the way that boiling water does. The brain even of a new born child is made so firm by it, as it can be handled with great freedom. The crystalline and vitrous humours of the eye become firm, but white and opaque, by soaking in it. The liquor of sebaceous glands, of mucous folliculi, the seed, &c. are coagulated by it. The lymphatic, or watery liquors, such as the aqueous humour of the eye, water of the pericardium and amnios, are not changed by it. It heightens the red colour of injected liquors so much, that vessels, which did not at first appear, are plainly seen, after the part has been in it some time. If you'll compare these effects with what Ruysch has said here and there of his preparations, you'll find that the liquor described comes pretty much up to the properties of his balsam, as he calls the liquor in which he preserved his wet preparations *.

The proportion of the acid to be mixed with the ardent spirit, is to be varied according to the part to be immersed in it, and the design the anatomist has. When the brain, humours of the eye, &c. are to be coagulated, a larger proportion

* Ruysch's balsam, according to Rigerus, (*Notit. rerum natural. et artefact*) is rectified spirit of malt, *frumenti*, to which a third part of water is added. Ruysch distilled this spirit in a tin alembic, with a slow fire; and thought that the addition of a handful of black pepper made the spirit penetrate better into the fleshy parts.

portion of acid is necessary, such is, two drachms of spirit of nitre to a pound of alcohol. When the parts are only to be preserved, especially if there are any bones in the preparation, forty, thirty, or fewer drops of the acid will be sufficient. If too large a proportion of the acid is mixed, the bones become first flexible, and then dissolve.

After any part is embalmed, particular care must be taken to keep it always covered with the liquor, otherwise its colour spoils, and some parts harden, others dissolve. To prevent therefore the evaporation as much as possible, and to exclude the air, which makes the spirits extract a tincture, the mouth of the glass is to be carefully stopped with a glass or a waxed cork, cover, or stopper; over which leaf-tin, a bladder, or injected membrane, is tied, in which way it will keep a considerable time without any great waste. Whenever the spirits sink near to the top of the preparation, more of the spirit of wine must be added, without the acid, which flies little off. If at any time the spirits acquire too high a tincture, they must be poured off, and new spirits, with a less proportion of the acid than at first, must be put in their place, the old ones being kept in a well corked bottle, for washing away the natural liquors of any new preparation, which ought always to be carefully done before any part is embalmed, and the old tinctured spirits are likewise to be washed off, with a small quantity of limpid spirits, every time the old ones are renewed, or a new preparation is to be preserved. The spirits unfit to be longer shewn in glasses,

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may alſo be ſtill employed in preſerving parts in earthen veſſels or glaſs ones, out of which the preparation muſt be taken when it is to be viewed.

It may not be amiſs to remark here, that the glaſſes in which preparations are to be demonſtrated, ought to be of the fineſt moſt tranſparent thick kind, for theſe allow the parts to be ſeen moſt diſtinctly, without changing their colour, and at the ſame time magnify the objects, diſcovering parts, which, when they are out of the glaſs, could not be ſeen with the naked eye. Since then the glaſs with the liquor has a certain focus, where objects are ſeen moſt diſtinctly, it will be neceſſary to uſe ſome contrivance for keeping the preparation at the proper diſtance from the ſide of the glaſs; which may be done by putting in a ſmall branch of ſome plant, or a little ſtick, or by faſtening the thread or hair by which the preparation is ſuſpended towards one ſide of the glaſs. Such little art, for keeping the parts expanded, or placing them ſo as to be ſeen to the beſt advantage, will eaſily occur to any who practiſes anatomy.

I ſhall finiſh this eſſay with a caution to anatomiſts to ſhun as much as poſſible putting their fingers into this acidulated ſpirits, or handling the preparations that are very wet with them; for they bring ſuch a numbneſs on the ſkin for ſome time, that makes the fingers unfit for any nice diſſection. The beſt cure I have uſed for this numbneſs, is waſhing the hands with water, into which a few guts of *ol. tartar. per deliquium* have been dropped.

XI. *Some*

XI. *Some Thoughts concerning the Production of* animal Heat, *and the Divarications of the vascular System, being an Abstract from a* Latin *Treatise of* the Heat of Animals : *In a Letter to Dr* JOHN STEVENSON *Physician in* Edinburgh, *from Dr* GEORGE MARTIN *Physician in St* Andrew's.

*SIR,*

I Cannot but acknowledge the pleasure I had from the good opinion you conceived of my treatise, *de Calore Animalium.* The objections you was pleased to propose against it, gave me full proof you had condescended to read and consider it carefully ; and at the same time gave me an opportunity, if not to obviate them entirely, at least to render the scheme somewhat less exceptionable. You think it would be of use to the world, and wish it were made public : But I do not yet think it finished ; and I know not if any works of mine shall ever arrive at that degree of perfection, as to be able to bear the light, and face the public by themselves. In the mean time, in compliance with your desire, which I shall always very much regard, I will make a short extract of some of the most material things of it, which, after revising, you may dispose of in any way you think proper.

I need not say much concerning the universality of this property of heat, whereof all animals, while in life, have a considerable share. Nor is it necessary to recount the sentiments

timents or miſtakes of others about it: It may ſuffice to declare my aſſent in general to the opinion now moſt commonly received, that the heat of animals is produced by the motion of the blood in the vaſcular ſyſtem.

2. Nor does it ſeem to owe its origin to any inteſtine motion of the particles of the flowing blood. When I view the circulation in the ſmall pellucid veſſels of animals, I ſee no inteſtine confuſion of particles; nor can I perceive any motion fit for the production of animal heat, except the progreſſive courſe of the blood preſſing and rubbing upon the ſides of the veſſels.

3. Nay, I am afraid, that even this motion of the blood, however rapid in the great trunks, will, in the minute veſſels, be deemed too ſmall and inconſiderable to produce an attrition ſufficient for the generating any ſenſible heat: So that it may be thought neceſſary to have recourſe to chemical principles; and to ſuppoſe heat to be a ſort of animal proceſs producing a certain change in the aliments, ſome way analogous to the luctas and efferveſcencies we obſerve many other bodies, in certain caſes, to undergo.

4. But it was juſt now remarked, that we could not perceive any violent inteſtine motion of the particles of the blood, viſible in the ordinary heating efferveſcencies. And beſide, in direct confirmation of the mechanical force of the attrition of the fluids on the ſides of

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the containing veſſels, from medical obſervations we find, in the various circumſtances of the human body, the heat generally in ſome ſort to correſpond to the degree of motion of the blood. And above all, I hope I ſhall be able to make it appear, that the vaſcular ſyſtem is ſo admirably contrived, and the various motions of the blood ſo ſkilfully adjuſted by the great and wiſe Author of nature, that, bating external influences or diſturbances, the heat of the circulating fluids generated by attrition, is every where preſerved nearly uniform, with however differing celerities they be propelled in different parts of the animal machine.

5. In order to the clearing and ſettling of this, I muſt premiſe a general ſuppoſition, that the intenſity of heat generated by attrition is, *cæteris paribus*, in proportion to the relative celerity wherewith the bodies rub againſt one another.

6. And therefore, if a liquor be forcibly propelled through a canal, the quantity of attrition of the liquor upon the ſides of the canal generating heat, is in a compound ratio of the celerity of the liquor (C), and of the circumference (or diameter D) of a ſection of the canal; that is, always as $C \times D$.

7. When a liquor moves through a canal, its particles, tho they have no briſk inteſtine motion, are frequently, however ſlowly, ſhifting places. Whence we are to conceive the quantity of attrition around the circumference

of a section, as equably diffused through the whole section, so as to render the heat in every point of it from the circumference to the center perfectly uniform: And therefore the real intensity of heat in such a section is found to be as the quantity of attrition (C × D) applied to (Z) the area of the section, or as $\frac{C \times D}{z}$; that is as $\left(\frac{C \times D}{D^2}\right)$ the velocity of the liquor, and the diameter of the canal directly, and the square of that diameter reciprocally. Which comes out as $\left(\frac{D}{C}\right)$ the velocity of the liquor applied to the diameter of the canal.

8. And from this it easily follows, that, if liquors be propelled with celerities proportional to the diameters of their containing canals, the heat of these liquors generated by attrition shall be equal. For D, *d* being the diameters of the canals proportional to C, *c* the celerities of the propelled liquors; in this case C . . *c* : : D . . *d*, and therefore C . . D : : *c* . . d; and consequently $\frac{C}{D} = \frac{c}{d}$; hat is, the respective intensities of heat are equal.

9. If a canal carrying a liquor be ramified into branches, the heat of the liquor generated by the lateral attrition may be constantly equal; or may be increased or diminished in its progress through this ramified system of vessels, accord-
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ing to the method of its divarication, or the proportion that the wideness of the branches bears to the wideness of the trunks from whence they arise.

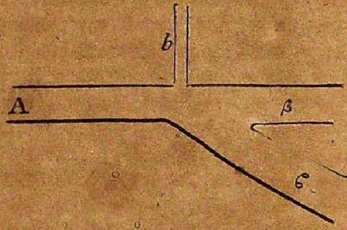
' For the heat of the propelled liquor generated by attrition is every where as its celerity applied to the diameter of the canal through which it is carried; so that if, in the divarication of a canal, the branches have always such a situation and largeness with respect to the trunk from whence they arise, that the velocities of the liquor moving through them shall always correspond to their diameters, then the heat generated by attrition shall be constantly equal.

But, if the largeness of the branches be so adapted to the capacity of the trunks, that the velocity of the liquor flowing in them shall be greater, equal, or even but nearly equal to that in the trunks, then the heat of the liquor generated by attrition would be constantly increasing in its progress through this supposed ramified system, and that, *cæteris paribus*, in a reciprocal proportion of the diameters of the canals.

And, on the other hand, if the capacities of all the branches put together should be vastly greater than the largeness of the trunk from whence they arise, so as to diminish the velocities of the liquor in a greater proportion than the diameters of the canals; then indeed the heat in such a system would be constantly on the decay.

10. It seems to be the first of these cases which actually obtains in the animal machine. For,

For, though the antients made a great ado about the various degrees of heat of the different parts of the human body; yet some of the best and most careful observers amongst the moderns, by methods more certain and regular than what the others were masters of, have been assured that they are all nearly of the same degree of heat, when left to themselves in a sound state, and sufficiently defended from the injuries of the air, or external cold; which I have confirmed by a thousand experiments: And therefore it behoves us to inquire more narrowly into the nature of such a divarication, which may render the velocity of the blood always in proportion to the diameters of its containing canals, and consequently give it a heat constantly equal.




11. Suppose any artery A to be divided into what number of branches $b$, $\beta$, $\epsilon$, you please, equal, or however unequal, whose diameters we call $d$, $\delta$, $\Delta$, the diameter of the trunk A being D: So that their respective orifices shall be proportional to $D^2$, $d^2$, $\delta^2$, $\Delta^2$; while the celerities C, $\gamma$, $\varkappa$, $k$, wherewith the blood is supposed to flow through these vessels, are required to be proportional to the foresaid diameters D, $d$, $\delta$, $\Delta$ respectively.

Now, in the divarication of any artery, the blood

blood would flow into the ſeveral branches with the ſame, or nearly the ſame celerity, if they all roſe with the ſame obliquity to the trunk from whence they ſpring. Therefore it ſeems neceſſary, for the due balancing of the velocity, to contrive the divarication in ſuch a manner, that the larger branches ſhall always lie in a directer courſe, and the ſmaller ones in a more inclined one to the current in the trunk, that the quantities of the blood flowing into them may be in a greater ratio than their orifices would otherwiſe receive; ſo too that it may flow through them with the requiſite velocities proportional to their diameters.

12. Thus then, the poſition of the branches being ſettled, we come next to inquire what proportion the amplitude or orifice of the trunk muſt have to the conjunct amplitudes of the branches, ſo likewiſe as to preſerve the above mentioned balance of celerity. In which inquiry, in the firſt place, we are to obſerve, that, univerſally, the quantities of liquor paſſing equably in a given time through any canals, are in a compound ratio of the amplitudes of the canals, and of the velocities of the liquor conjunctly. Hence then the quantities of blood paſſing in a given time through the branches, $b$, $\beta$, $\varsigma$, are as $d^2 \times c$, $\delta^2 \times \kappa$ $\Delta^2 \times k$ reſpectively; to all which quantities, the quantity furniſhed them by the trunk A, or paſſing through the trunk in the ſame time, muſt be equal, and as $D^2 \times C$ That is, then $D^2 \times C = d^2 \times c + \delta^2 \times \kappa + \Delta^2 \times k$. And therefore, in the preſent caſe, (the celerities $C$, $c$,

C, *c*, *z*, *k*, being ſuppoſed proportional to the diameters D, *d*, δ, Δ) $D^3 \times D$ ſhall be equal to $d^2 \times d + \delta^2 \times \delta + \Delta^2 \times \delta$; or $D^3 = d^3 + \delta^3 + \Delta^3$. That is, *the cube of the diameter of the trunk is equal to the cubes of the diameters of all its branches added together.* And ſo D is equal to $\sqrt[3]{d^3 + \delta^3 + \Delta^3}$; that is, *the diameter of the trunk is equal to the cube-root of the conjunct cubes of the diameters of all the branches.*

From whence, if once we know what proportion the branches have to one another, we can for certain determine what ratio they, when put together, muſt have to the trunk from whence they ariſe. Which was the thing propoſed to be found out concerning the nature of that divarication, where the celerities of the flowing liquor are ſuppoſed always proportional to the diameters of the containing canals.

13. To illuſtrate this doctrine of the proportions of the trunks and branches of a ſyſtem of veſſels ramified according to the propoſed law of divarication, for preſerving the velocities of the contained liquor in a conſtant ratio to the diameters of its containing canals, and ſo to maintain a conſtant uniform degree of heat, let us try the reſult of it in two or three familiar examples.

If a trunk ſhould divide itſelf into two equal branches, whoſe orifices ſhall be to one another, as 1 to 1, and their diameters in like manner to $\sqrt[2]{1}$ and $\sqrt[2]{1}$, or 1 and 1; then, by

by the general propofition § 12. the diameter of the trunk is proportional to $3\sqrt{1^3 + 1^3} = 3\sqrt{2} = 1'2599$; and confequently its orifice muft be $1'2599^2 = 1'5874$. So that the amplitude of the trunk is to the conjunct amplitudes of the branches as 1'5874 to $1 + 1 = 2$; or as 100 to 125'99.

Suppofe a trunk divided into two, however unequal branches, which, for example, fhould be to one another as 2 to 1; fo that the diameter of the greater branch fhall be proportional to $2\sqrt{2} = 1'4141$, that of the leffer as $2\sqrt{1} = 1$. Then the diameter of the trunk is proportional $3\sqrt{1'414^3 + 1^3} = 1'5644$; and its real largenefs, compared to the branches 2 and 1 fhall be $1'5644^2 = 2'4473$. So that this amplitude of the trunk, being to the joint amplitudes of the branches as 2'4473 to $2 + 1 = 3$, fhall be to them likewife in the ratio of 100 to 122'586.

But, fhould the divarication be more complex, fo that there be three, and thefe very unequal branches, in the ratio perhaps of 3, 2, 1; whofe diameters therefore muft be proportional to $2\sqrt{3} = 1'73205$, $2\sqrt{2} = 1'4141$, and $2\sqrt{1} = 1$: Then the diameter of the trunk comes out proportional to $3\sqrt{1'73205^3 + 1'414^3 + 1^3} = 3\sqrt{9'02395} = 2'0819$, the fquare whereof is 4'3344. Whence we find the trunk to be to the fum of the branches put together as 4'3344 to $3 + 2 + 1 = 6$; that is, as 100 to 138'42.

14. I

14. I obſerved before, § 10. that the heat in different parts of the body, in a natural and ſound ſtate, was every where nearly equal. And now having determined the laws of the divarication of a ramified ſyſtem of canals wherein the heat generated by attrition ſhould be conſtantly uniform; let us, in the next place, inquire, if theſe laws obtain in fact; and if anatomy and a true menſuration of the veſſels of the human body do in reality correſpond to our theory.

It is obvious at firſt ſight, and every body may have obſerved, that the poſition of the veſſels intirely favours our ſcheme. Is it not manifeſt, as it has been ſometimes very juſtly remarked, that the greateſt veſſels lie more directly in the way of the trunks from whence they ariſe, while the ſmall ones riſe at very great inclinations according to their various ſizes? (the like whereof may, in ſome meaſure, be obſerved even in the ramifications of trees and other vegetables) from whence the blood, flowing more freely into the greater branches, muſt likewiſe paſs through them with a proportionably greater velocity, than through the minuter veſſels, which give not ſuch an advantageous ingreſs to the pretruded fluid.

15. The next thing we are to conſider is the ſize of the veſſels, or the proportions of the branches to their furniſhing trunks. It is very difficult, if not impoſſible, to ſurvey the veſſels of animals with a mathematical exactneſs

nefs; so as to find them always observing the same rule. In measuring an artery, for example, a line broad, who will undertake never to err $\frac{1}{200}$ part of an inch? And yet an error so seemingly little will lead us into a mistake near $\frac{1}{8}$ of the amplitude of the vessel: So that the smallest unavoidable errors in measuring shall occasion some seeming deviations from the rules that really nature may have followed. And therefore I hope even the most scrupulous will be satisfied, if, upon the strictest examination, it shall be found that experience and our theory, though not always jumping exactly in the same numbers, are, however, for the most part, very little wide of one another. Physical experiments and practical mechanics allow only of an approximation to geometrical demonstrations.

Now, it has been observed in general by others, that the conjunct amplitude of the branches of arteries are always larger than the trunks from whence they arise; and consequently that the blood, on this account chiefly, suffers a vast retardation in its progress from the heart to the extreme parts of the body. But we do not stop in this general observation: From innumerable measures and experiments, we moreover pretend to find a determined proportion, and a very elegant harmony in the dilatation of the arterial system, and in the retardation of the blood moving through it; to wit, That the diameter of every artery is equal to the cube-root of the conjunct cubes of the diameters of all its branches: And the velocity of the blood in the arteries always in proportion

proportion to their feveral diameters, for the prefervation of an equable degree of heat thro' all this far extended fyftem.

16. This, I fay, we find from the niceft examination of the veffels that poffibly can be made; the feveral meafures fometimes exceeding, fometimes not coming intirely up to the rule; but by fo little differences, and thefe as often on the one fide as the other, that we cannot doubt of fuch fmall variations having flowed from fome overlooked circumftance, or fome inevitable errors of menfuration: Which is plain from the following very ample collection of experiments, wherein, at one view, we have the proportions of the trunks to the branches, both according to the theory, and according to the actual meafures that were taken of them, with the differences in thefe two circumftances. Which differences are truly lefs than we could well have expected; and which would ftill have been but a half of what we have here fet down, if we had made our calculations of the diameters, and not of the amplitudes of the veffels: And they are the diameters which are primarily meafured.

17. No body will blame me that I have, in confirmation of this doctrine, chofen to give other people's experiments rather than my own. Thofe are liable to no objections: The authors of them could have no prepoffeffions in favour of a fcheme they were not apprifed of; which I acknowledge would be much more exceptionable, had I founded it only on my own obfervations.

In

In the firſt place, I have ſet down ſome meaſures taken with all the care imaginable from the anatomical tables of Euſtachio; who, of all the anatomiſts, ſeems to have ſtudied, with the greateſt accuracy, the ſymmetry of the human body, and the juſt proportions of all its parts. However, that I may not conceal any the leaſt material circumſtance from you, I muſt acknowledge, that, in comparing the aorta with the iliacs, I took it where it appears ſmalleſt in his figures, which is a little below the emulgents: Which trunk, beſide the iliacs, gives off other arteries, as ſome of the lumbars, the inferior meſenteric, and the ſacra. But theſe, when taken in too, are not really of ſuch conſideration as much to diſturb or alter our calculations.

18. Then follow ſome meaſures taken from Profeſſor Ruyſch's figures. His ſingular art in injecting the veſſels, and the great pains he took to have them accurately delineated, every body knows.

But ſince, whatever Euſtachio and Ruyſch took, it muſt have been impoſſible to ſhun ſome ſmall deviations from nature, both in painting and graving their tables, (as we find ſome variations in the ſame figures repeated in different tables of Euſtachio), and that we likeways may not have meaſured them with abſolute and perfect exactneſs, you will not wonder that they do not preciſely coincide with the numbers of the theory. However, this ſtill comes out as a medium between them.

19. The experiments we have borrowed from

from the ingenious Dr Keill are liable to none of the inconveniencies of painting. He mea-ſured the veſſels themſelves; and therefore I have adopted all his meaſures: Only ſome few I have omitted, which, for their being taken from too minute veſſels, could not be much relied on; and two or three more, which, by tranſgreſſing the neceſſary laws of ramifica-tions, as omitting a branch or two, or exhi-biting a branch greater than its furniſhing trunk, diſcover ſome miſtake to have crept into theſe obſervations. On which account, and for other like reaſons, I have neglected moſt of the ordinary anatomical figures: However, even ſome of theſe I have now likewiſe added, under the title of *Miſcellany obſervations;* though I do not pretend to lay ſo much ſtreſs on them, as upon the other experiments I had firſt taken in, eſpecially thoſe of Keill and Eu-ſtachio.

20. That I might not ſeem to have neglect-ed ſuch meaſures myſelf, for the greater con-firmation of the reſt, if any thing of mine could add to their authority, I have added ſome of my obſervations too. And, being unwilling to omit any thing of this kind, in tranſcribing this paper, I have inſerted two or three that the induſtrious Dr Nichols has now furniſhed us; who, I find, has been at more than ordinary pains in ſurveying the veſſels both of animals and vegetables.

21. And now follows the collection I pro-miſed of the experiments themſelves, ſhewing the proportions of the trunks to their branch-es in the arterial ſyſtem of the human body.

22. In

22. In this collection there are indeed but few observations which perfectly jump with the exact proportions of the theory: But many come very near them; and the numbers of those coming short of it, and of those that exceed it, are nearly equal. And the common or middle excess or defect, (reckoning one with another) is only about $\frac{1}{14}$ part. Nay, in about ninety observations, from the excesses and defects balancing one another, the sizes of the branches determined by our rules, and those found out by the most carefully made experiments, come out almost exactly the same. The odds being about $\frac{1}{600}$ or $\frac{1}{700}$ part; a quantity in such a case to be esteemed as nothing. A surprising coincidence! and which, before trial, we durst not in such difficult and precarious measures have expected or promised upon.

23. Seeing then those observations quadrate so nicely to theory, even more exactly than the most scrupulous would have required, I must acknowledge the very sensible pleasure I had in perceiving this beautiful harmony; and that I was exceedingly delighted to find (as I wished and expected) the arteries of the human body to be distributed and divaricated in such a manner, both in their position and wideness, that the celerity of the blood flowing through them may be preserved in a given proportion to their diameters; and the same, or nearly the same degree of heat generated

 by

by attrition, continued along the whole arterial ſyſtem.

24. To obviate ſome difficulties that might be ſtarted againſt us, we muſt obſerve, that, for the eaſier reception of the blood, the artery is always a little wider in its riſe from the trunk, from whence it converges in a conical form: And that arteries, conſidered as trunks, juſt before they ſplit into branches, muſt be widened a little for the more convenient divarication. So that, if an artery, from the beginning of its riſe to its ramification into branches, happen to be very ſhort, it will commonly be found wider, and have a greater proportion to its branches than our theory would require.

Thus, for example, the great trunk of the right ſubclavian, before it divides into the carotid and axillary, is ſometimes longer, and ſometimes ſhorter. In the firſt caſe, as in Euſtachio's Tab. XVI. Fig. 1. or Ruyſch's Epiſt. Probl. III. Tab. III. Fig. 2. it almoſt coincides with the theory. In the ſecond caſe, as in Ruyſch's ibid. Fig. 3 and Cowper's Anat. hum. bod. Append. Tab. III. it differs ſo far from our rules, as even to be much larger than the conjoined branches which ariſe from it. Thus too, the great trunk of the aorta being very ſhort, and alſo curvated, is found to have a greater ratio to its primary branches, than otherwiſe we ſhould have had reaſon to expect: And ſo it ſeems Valſalva * reckoned it as a ſort

* See Comment. acad. Bonon. p. 376.

ſort of ſinus. In which, and other like caſes, though we allow the blood to have a ſlow courſe through the trunks, we need not be apprehenſive of any loſs or decay of heat in ſuch ſhort intervals: Which too may ſoon be ſufficiently compenſated by the ſubſequent more regularly adjuſted arteries; eſpecially that the ſmaller veſſels do frequently, by inoſculations, communicate one with another; and ſo, beſide other uſes, help to maintain a due and regular balance of the velocity of the circulating fluids.

25. And, on the other hand, for the like reaſons, we need not be afraid of the heat being too much increaſed, though it ſhould move ſomething faſter in the end, than in the beginning of the long converging arterial trunks, as in the carotids or iliacs; which run a good ſpace before their divarication, without emitting any conſiderable branches.

26. Contrary to what I alledged of the larger branches riſing more directly, and the ſmall ones obliquely to the courſe of the blood, it will be ſaid that ſome, even pretty large veſſels, riſe at an acute angle from their trunks in a retrograde way; ſuch as the umbilical arteries in a fœtus, or the epigaſtrics from the external iliacs. This I acknowledge one will be ready to think from inſpecting the figures in the common anatomical tables. But every body that is acquainted with diſſections, muſt have obſerved, that theſe tables, either for the convenience of diſſecting or painting, or even ſome-

sometimes, by the carelessness of the authors, do very often exhibit things far out of their natural situation. And, in this particular case before us, if we look into the book of nature itself, and examine things as the great Author thereof has designed them, we shall find, that, though such vessels do carry the blood in a course directly contrary to its flux in the trunks, yet their rise is much in the same angle with others of the like size. Thus the epigastric artery, instead of rising at an acute angle from the outer side of the external iliac, as Vesalius and the ordinary tables represent it, does really spring from the inner side of that artery (as in Cowper's delineation, ibid.) at an angle nearly a right one, but somewhat obtuse; and then forming an arch, climbs upwards, carrying the blood in a course retrograde to what it was in before; which Eustachio * alone has taken care justly to represent, as he is the anatomist in the world who has most carefully studied to exhibit all the parts undisturbed, and in their due and regular posture.

It remains now to apply our rule of the ramification of vessels to some other purposes in the animal oeconomy; and to consider this doctrine with respect to different animals; and to inquire what alterations of heat, if any, their difference of magnitude will produce: How it may be influenced by any other changes,

* Tab. Anat. Tab. XIII. XIV. Fig. 1. Tab. XXV. XXVII. Fig. 12.

changes, whether in the ſolids or fluids, as by the non-naturals, age, ſex, &c. But all this would be difficult to abridge; and to tranſcribe all, would carry me far beyond the bounds I had fixed to myſelf at this time.

XII. *An Eſſay concerning the Motions of our eyes; by* WILLIAM PORTERFIELD, M. D. *Fellow of the College of Phyſicians at* Edinburgh.

## PART I.

## OF THEIR EXTERNAL MOTIONS.

THE motions of the eye are either external or internal. I call *external*, thoſe motions performed by its four ſtraight and two oblique muſcles, whereby the whole globe of the eye changes its ſituation or direction. And, by its *internal* motions, I underſtand thoſe motions which only happen to ſome of its internal parts, ſuch as the cryſtalline and iris, or to the whole eye, when it changes its ſpherical figure, and becomes oblong or flat.

In this paper I ſhall only treat of its external motions, reſerving the internal motions for the far more fertile ſubject of another eſſay, which I intend to ſend you hereafter, if I find that your readers have any reliſh for ſuch inquiries.

The ſpherical figure of our eyes, and their looſe connection to the edge of the orbit by the *tunica conjunctiva*, which is ſoft, flexible, and

and yielding, does excellently diſpoſe them to be moved this or the other way, according to the ſituation of the object we would view. This membrane is from its ſituation alſo called *adnata*. It takes its origin from the perioſteum all round the edge of the orbit, and is extended over the whole fore part of the globe, till its termination in the edge of the ſclerotis, where it joins the cornea. It is called *conjunctiva*, from its office, *quia oculum cum capite conjungit*.

This membrane is covered externally with another membrane: For, as is known to anatomiſts, the internal membrane of the eye-lids, at the edge of the orbit, is turned forwards upon the outward face of the eye, and is coextended over it with the *tunica conjunctiva*, to which it is adherent.

Theſe two membranes, becauſe of their cloſe union, appear to be only one, and are generally deſcribed as ſuch under the name of *membrana albuginea*, ſo called, becauſe they form the white of the eye, though in fact they are diſtinct membranes, eaſily to be ſeparated; the one a continuation of the perioſteum, lining the orbits internally, and the other of the inner membrane of the eye-lids. Theſe membranes, eſpecially the external, are ſo full of blood-veſſels, and ſo laxly extended, that in violent ophthalmia's the white of the eye is ſometimes ſwelled ſo exceſſively, as to cover all the cornea, which I here take notice of, becauſe it is ready not only to ſurprize, but to impoſe upon the unwary or unexperienced oculiſt,

culist, as if it were an incurable excrescence of the cornea itself.

Besides these two membranes, the fore part of the globe is covered all over externally with a very thin transparent aponeurose or surpeau, which not only covers the membrane which it has from the eye-lids, but likewise is extended beyond it over the cornea itself. The phlyctaenae, which are small transparent vesicles full of clear water, and which are frequently observed upon the surface of the cornea itself, as well as upon the white of the eye, and even sometimes have their centre in some part of that circle of the cornea, where it joins the sclerotis, and by that means occupy at the same time both a part of the white of the eye, and a part of the cornea, are, amongst other things that might be advanced, a convincing proof of the existence of this surpeau, and of its extension over the whole cornea.

It is by these membranes that the eye is connected to the edge of the orbit, which, being soft and flexible, they do in such a manner, as not in the least to impede its necessary motions. And besides, there is a great deal of fat placed all round the globe, betwixt it and the orbit, which lubricates and softens the eye, and renders its motions more easy.

Now, the external motions of the eye are, as we before hinted, performed by means of six muscles, whereof four are straight, and two oblique. Gabriel Fallopius (in his *Observationes anatomicæ)* is among the first that has given us a genuine description of the muscles of the eye: For before him not only Galen but Vesalius

lius himself has grossly erred in the description of the oblique muscles, and in assigning seven muscles to the human eye; on which account Realdus Columbus (*de re anatom. lib. 5. c. 8.*) does indeed justly reprehend them, though, at the same time, he commits no less an error himself, not only in supposing that the *obliquus inferior* begins and ends in the cornea of the eye, but also in imagining, contrary to what Galen and Vesalius teach, that the *obliquus superior* belongs to the eye-lids.

The first of the four straight muscles is situated upon the superior part of the globe upon which it lies. It pulleth up the eye when we look up, and is therefore called *attollens* or *superbus*, it being one of the chief marks of a haughty disposition to look high; wherefore its opposite muscle is called *humilis*. But Casserius Placentinus thinks the motion of the upper eye-lids denotes these dispositions more significantly; for, says he, (lib. 5. cap. 18.) "Qui enim hanc elatam habent" (speaking of the upper eye-lids) "superbi et feroces sunt; qui "vero depressam ac dimidium fere oculum claudentem, ita ut terram adspicere videantur, humiles et mites sunt." For which reason Willis (in his *anima brutorum, cap. 15.*) chuses rather to call them "pii aut devoti: Quia in precatione intensa," says he, "oculum valde attollunt; quare hypocritis, qui sanctitatis speciem "affectant, in more est, oculum ita evolvere, ut "albo fere tantum conspecto pupilla occultetur."

The second, as before hinted, is directly opposite to the attollens, and is situated upon the under part of the eye which it pulls

down

down, and is therefore called *deprimens* or *humilis*.

The third and fourth are towards the sides of the eye, and draw it towards the nose, or from it towards the little angle. That which draws it towards the nose is called *abductor* or *bibitorius*, because, in drinking, the eyes are turned inwards to the great angle for viewing the drink. That which pulls it from the nose towards the little angle, is called *abductor, or indignabundus*, because it is made use of in those lateral or squint views that denote a scornful resentment.

All these four muscles arise from the circumference of the hole in the bottom of the orbit, through which the optic nerves pass; and, advancing by the four cardinal parts of the eye, terminate by the four broad thin tendons in the sclerotis.

These tendons form a large aponeurose, which is spread over the outward face of the eye under the conjunctiva, to which it also adheres, and terminates at the edge of the sclerotis, where it forms the cornea. Columbus pretends to be the first discoverer of this tunicle, to which he has given no name. Hence it is frequently named *tunica innominata Columbi*, tho' unjustly, because it was known to Galen, as appears from the 2d and 8th chapters of his 10th book *de usu partium*. Others therefore with better reason call it *tunica tendinea*, because formed of the tendons of the four straight muscles. Aquapendent is of opinion, that the white of the eye has its colour from this membrane; But the conjunctiva, and the

 tunicle

tunicle which comes from the inner membrane of the eye-lids, do likewise concur, as has been demonstrated by Plempius (*Ophthalmographia, lib.* 1. *cap.* 8 ).

When the four straight muscles of the eye act separately, they pull the globe up or down, to or from the nose, according to the different situation of objects we would view. But, when the superbus and adductor or abductor act together, or when the humilis and adductor or abductor act together, they perform the oblique motions, which have been attributed to the oblique muscles; and, when all four act together, they draw the eye inwards towards the bottom of the orbit, and keep it fixed in an equal situation, which is therefore by physicians called its *tonic motion.*

Some are likewise of opinion, that when all these four muscles act together, the bulb of the eye is compressed, and its axis is lengthened, when objects are too near us; while others give them a quite contrary action. But this we only mention by the way, reserving it to be further considered when we come to examine the inward motions of this organ.

The oblique muscles of the eye are two in number, whereof one is called *obliquus major* or *superior*, the other *obliquus minor* or *inferior*; they receive their denomination from their oblique position and course.

The *obliquus major*, because of its length, is sometimes called *longissimus oculi*; it arises from the edge of the hole in the bottom of the orbit that transmits the optic nerve, between the elevator and adductor, from whence it runs obliquely

obliquely to the great canthus: In the upper part of which, near the brink, there is a cartilaginous ring or trochlea affixed to the *os frontis*, through which it paſſes its tendon; from whence turning backwards, it is inſerted into the *tunica ſclerotica*, towards the back part of the bulb of the eye, in the middle of the diſtance between the termination of the attollens and the optic nerve.

The trochlea through which this muſcle paſſes its tendon, was firſt diſcovered by the great Fallopius, who therefore juſtly receives the honour due to ſuch a diſcovery; tho' Riolanus does likewiſe aſcribe it to his contemporary Rondeletius. From it ſometimes the muſcle receives its name, and is called *trochlearis*: When it acts, it rolls the eye about its axis towards the noſe, and at the ſame time draws it forward, and turns its pupil downwards.

The ſecond of theſe oblique muſcles, becauſe of its being the ſhorteſt muſcle of the eye, is frequently deſcribed under the name of *breviſſimus oculi*. It takes its origin from the lower part of the orbit in its inſide near its edge; and aſcending obliquely by the outer corner of the eye, it is inſerted into the ſclerotis near the implantation of the former, directly betwixt the abducens and optic nerve.

The action of this muſcle is to roll the eye about its axis from the noſe, and at the ſame time to draw it forwards, and direct its pupil upwards.

These two oblique muſcles are by ſome called *circumagentes* and *amatorii* (*amoureux*) from

from their actions in winding and rolling the eye about, which motions we call *ogling*. But the French academist Mr Perrault (*du mouvement des yeux*) will not allow that the eyes have ever any motion round their axis, becauſe he could never obſerve it in the eyes of tortoiſes, which have ſome fixed ſpots that may ſerve for rendering ſuch motions obvious; but chiefly becauſe he does not ſee what advantage we could reap therefrom. But, were nature to be confined and limited in her operations by our imperfect views of the advantages of her actions, we ſhould frequently deny the moſt evident facts in the world.

But that Perrault's authority may not miſlead ſuch as have not accurately obſerved the origin, progreſs, and inſertion of theſe muſcles, it may be proper to obſerve, that the learned Mr Mariotte (in his *nouvelle decouverte touchant la vüe*) has demonſtrated beyond diſpute, that that part of the bottom of our eyes, where the optic nerves enter them, is inſenſible; and that the rays of light, which fall thereon, are entirely loſt, without giving us any idea of the object from whence they came. Now our optic nerves enter the eye, not in the middle oppoſite to the pupil, but a little on the inſide towards the noſe. Hence objects placed a little on the outſide of the optic axis, if not over large, would be altogether inviſible, becauſe the rays which come from them fall upon that inſenſible part of the bottom of our eyes, at which the optic nerves enter; but, by the circumrotation of our eyes round their axis, this inſenſible part

part may be turned aside, and the rays of light which would have been lost, in falling upon it, may now, at least in part, fall upon the sensible part of our retina; and therefore the object, which otherwise would have been entirely invisible to that eye, may, at least, in part, become visible, which is a considerable advantage, as every one must see.

I am not ignorant that there are many who have denied this oblique insertion of our optic nerves. Willis and Briggs tell us, that not only in man, but also in dogs, cats, and all the more sagacious creatures, they enter the globe at its axis directly opposite to the pupilla: But the labour and industry of later and more accurate anatomists have long ago freed us from this mistake; and, though this obliquity is considerably less in man than in oxen, sheep, swine, and the greatest part of birds and fishes, yet no one, who shall take the pains to examine a human eye, can miss observing it.

There are indeed some creatures, such as the porcupine and sea-calf, that have the optic nerves inserted into the axis of their eyes: Which single fact more effectually overturns Mariotte's hypothesis of the choroides being the principal and immediate organ of sight, than all the subtile reasoning of Messrs Pecquet and Perrault, his greatest opposers. Neither is it possible that this defect in our sight, where the optic nerves enter, can arise from the want of the choroides in this place, which, according to de la Hire's reasoning against Mariotte (see his dissertation, *Sur les*

*les differens accidens de la vüe*) ought to receive the impreſſion from the rays of light (which, according to him, paſs through the tranſparent retina, without producing viſion) and communicate it to the retina, with that diſpoſition and modification which is proper for ſight, juſt as the ſpiral lamella of the ear receives the impreſſions of the air, to be communicated to the auditory nerve, for exciting in the mind the idea of ſound. For, were this true, then in theſe juſt now named animals, all objects would become inviſible, to which their eyes are directly turned, becauſe the choroides is wanting in that place where their image falls; which being contrary to experience, it remains that ſome other cauſe be aſſigned for that defect of our ſight than the want of the choroides. But, to return:

Tho' the action of theſe two oblique muſcles ſeems pretty evident, there is ſcarce any part of the human body about which anatomiſts have differed more, than in aſſigning them their proper offices. The famous Mr Cowper is among the firſt I know who began to reaſon juſtly about them. But it would take up too much time to enumerate and confute the ſeveral opinions of different authors; and therefore I ſhall content myſelf, after what has been already ſaid of each muſcle acting a part, to conſider what happens, when both act at the ſame time.

Mr Cowper, in his *Myotomia reformata*, has well obſerved, that when any of the ſtraight muſcles act, they will rather draw the eye inwards, within the orbit, than turn it either ſideways,

ſideways, or upwards, or downwards, were it not at the ſame time drawn outwards by ſome equal force. Now, the above deſcribed ſituation of theſe oblique muſcles excellently qualifies them for keeping the globe from being retracted, when any of its ſtraight muſcles act: For, by their joint contraction, they muſt pull the eye outward from the bottom of the orbit, and keep it ſuſpended as upon an axis, for the better receiving the motions of the ſtraight muſcles: And this is what we think the principal uſe of its oblique muſcles, when acting together, ſeeing they combine both in this, while they are antagoniſts to one another in their other actions.

Aquapendent (in his treatiſe *de oculo, cap.* xi.) obſerves, that, in the pike, the oblique muſcles decuſſate one another in form of a croſs; and Perrault (*du mouvement des yeux*) tells us, that they are both in the under part of the eye; and that becauſe, in ſuch rapacious animals, who frequently dive in purſuit of their prey, they have occaſion more than others to turn their eyes downwards. But this we chiefly take notice of, becauſe it may afterwards be of ſome uſe for determining how the eye changes its conformation, and adapts itſelf to the different diſtances of objects, which ſome have aſcribed to the action of thoſe muſcles.

Cowper (in his *Myotomia reformata*) quotes Mullinette, for deſcribing a ſeventh muſcle, which he calls the fifth right muſcle, whoſe office he confines to the motion of the trochlea. But, upon examination, no ſuch muſcle

is to be found in the human eye; and it is poſſible that Mullinette might be led into the miſtake, by that part of the *orbicularis palpebrarum*, which adheres to the trochlea, or rather by what he might have obſerved in dogs, who have a ſmall muſcle ariſing near the origin of the *obliquus major*, and inferting itſelf by a very ſlender tendon into the trochlea, to whoſe motions it is ſubſervient, as Douglas obſerves (*Myographia comparata, cap.* vi.)

Beſides theſe muſcles already deſcribed, quadrupeds are provided with another, commonly called *ſuſpenſorius*, from its aſſigned uſe in ſuſpending the eyes of ſuch animals as go much with their head hanging down towards the ground. This muſcle, among other things, diſcovers that Veſalius has not been altogether free from a fault, which he condemns very ſeverely in Galen, to wit, the obtruding on us the organs of brutes, inſtead of thoſe of the human body, which he pretends to deſcribe; for he has both deſcribed and painted it as belonging to man, in whom it is never found.

This muſcle ariſes from the circumference of the hole in the bottom of the orbit through which the optic nerve paſſes, and goes directly along the optic nerve, which it embraces and ſurrounds on all hands, and is inſerted into the back part of the ſclerotis, all round the optic nerve, betwixt it and the termination of the ſtraight muſcles. Fiſhes and fowls commonly want this muſcle, as well as man; but oxen, horſe, ſheep, hogs, and, ſo far as has been obſerved, all quadrupeds, are provided therewith, though, in all, it is not of the ſame ſtructure,

ſtructure, being ſometimes compoſed of two, three, or four diſtinct muſcles, as Aquapendent (*de oculo, cap.* xi.) obſerves.

Aquapendent, Willis, and Briggs, with the greateſt part of our modern anatomiſts, are of opinion, that the only uſe of this muſcle, is to draw the eye inwards, towards the bottom of the orbit, and to keep it ſuſpended, that, when the eye hangs down, as often happens in quadrupeds who gather their food from the ground, it may not fall too much out of the orbit, or by its weight ſtretch and fatigue the optic nerve to which it is attached. Hence they call it *ſuſpenſorius*, as has been before obſerved. But this action may in part be ſupplied by the ſtraight muſcles acting together; and, beſides, a ligament would have been ſufficient for ſuſpending the eye; and therefore it is probable that this muſcle has ſome other uſe.

Dr Tyſon, finding this muſcle in the porpeſs, as well as in quadrupeds, thinks its uſe is not to ſuſpend the bulb of the eye, but rather, by its equal contraction of the ſclerotis, to which it is affixed, to render the ball of the eye more or leſs ſpherical, according to the different diſtances of objects, concerning which you may conſult his Anatomy of the porpeſs, (p. 39.) But it is not abſolutely certain that the figure of the eye can be changed by the action of this muſcle, and that for reaſons afterwards to be mentioned, when we come to conſider its internal motions; and beſides, the neceſſary change of our eyes is well provided for by another mechaniſm, as will alſo appear in its proper place.

I

I think therefore that the use of this muscle is not only to suspend the eye, and preserve the optic nerve from being too much stretched, but principally to assist the straight muscles in moving the eye, according as its different fibres act, *e. g.* when its superior fibres act, they assist the attollens in pulling the eye up; when its internal fibres next the nose act, they assist the adducens; and, when both together, or those betwixt them, act, they pull the eye obliquely upwards towards the nose, and consequently assist the attollens and adducens in their joint action of moving the eye obliquely. Comparative anatomy makes this opinion very probable; for, in several animals, as we have before hinted, it is divided into several distinct muscles, whereof Aquapendent has observed sometimes three and sometimes four, in the eyes of sheep; and Douglas tells us, That in a dog it is divided sometimes into four, and sometimes into five, which have as many distinct insertions into the sclerotis. Mr Perrault's observation on this muscle does likewise very much confirm this opinion. (See his treatise, *Du mouvement des yeux*). His words translated are: "In effect we may say, (speak-
"ing of this muscle), that it contributes to the
"action of the straight muscles, according as
"its fibres act differently, there being several
"creatures, such as the bear, pole-cat, (*l'Ours,*
"*la Fouine*), and many others, where this
"muscle is separated into four, having as ma-
"ny different insertions, which being betwixt
"the insertions of the four straight muscles,
"may serve for the oblique motions of the eye,
"which

"which in man are chiefly performed by the "combination or fucceffive action of the four "ftraight mufcles."

Having examined what belongs to the mechanifm of the external motions of our eyes, I fhall now beg leave to add fome reflections thereon, which I flatter myfelf will not be altogether unacceptable to fome of your readers. And,

I. When nature has denied the head or eyes any motion, it is to be obferved that fhe has with great care and induftry provided for this defect. Dr Power's microfcopical obfervations furnifh us with a beautiful example of this: His words are: (Obfervat. 8.) "The "firft eminent thing we found in the houfe-"fpiders were their eyes, which in fome were "four, in fome fix, and in fome eight, ac-"cording to the proportion of their bulk and "the longity of their legs. Thefe eyes are "placed all in the forefront of their head, "(which is round and without any neck) all "diaphanous and tranfparent like a locket of "diamonds, or a fet of round cryftal-beads, "&c. Neither wonder why Providence fhould "be fo anomalous in this animal more than "in any other we know of, (Argus's head "being fixed to Arachne's fhoulders:) For, "1*ft*, Since they, wanting a neck, cannot "move their head, it is requifite that defect "fhould be fupplied by the multiplicity of "eyes. 2*dly*, Since they were to live by catch-"ing fo nimble a prey as a fly is, they ought "to fee her every way, and to take her *per* "*faltum* (as they do) without any motion of "their

"their head to discover her; which motion
"would have scarred away so timorous an in-
"sect."

It is therefore with good reason that Muffet, speaking of this Lydian spinstress, that proud madam, whom, for her rivalship, the fable makes Pallas transform into a spider, says of those philosophers that held them blind, "Sane "cœcutiunt illi summo meridie, qui videre ip-"sas non vident neque intelligunt:" Which he might have said with far better reason, if his eyes had been but assisted with one of our common microscopes.

To this purpose also belongs the surprisingly beautiful and curious mechanism observable in the immoveable eyes of flies, wasps, &c. they nearly resemble two protuberant hemispheres, each consisting of a prodigious number of other little segments of a sphere; all which segments are perforated by a hole which may be called their *pupil*, in which this is remarkable, that every foramen or pupil is of a lenticular nature, so that we see objects through them topsy-turvy, as through so many convex glasses; yea they become a small telescope, when there is a due focal distance between them and the lens of the microscope Leuwenhoeck's observations make it probable that every lens of the cornea supplies the place of the crystalline humour, which seems to be wanting in those creatures, and that each has a distinct branch of the optic nerve answering to it, upon which the images are painted; so that as most animals are binocular, and spiders for the most part octonocular, so flies, &c. are mult-

| The divaricated ARTERIES. | | Proportions of the Branches. | Capacities of the Trunks. | Conjunct Capacities of all the Branches. By the Theory. | Conjunct Capacities of all the Branches. By measuring. | Differences. | |
|---|---|---|---|---|---|---|---|
| From *EUSTACHIO.* | | | | | | | |
| The right Sub-clavian artery, divided into the axillary and carotid, ta[illegible] xvi. fig. 1. | | 11 . 12 | 100 | 125 | 129 | $\frac{1}{42}$ | + |
| A Mesenteric artery, the exactest way I could take the measures, tab. xi. fig. 1. | | 8 . 5 | 100 | 124 | 122 | $\frac{1}{62}$ | − |
| A Mesenteric artery in another subject, | tab. xxvii. fig. 4. | 1 . 1 | 100 | 126 | 119 | $\frac{1}{18}$ | − |
| The descending aorta split into the two Iliacs, | tab. xii. fig. 1. | 1 . 1 | 100 | 125 | 128 | $\frac{1}{63}$ | + |
| | tab. i. fig. 3. | 1 . 1 | 100 | 126 | 134 | $\frac{1}{16}$ | + |
| The same in another subject, | tab. xii. fig. 1. | 1 . 1 | 100 | 126 | 130 | $\frac{1}{32}$ | + |
| | tab. i. fig. 2. | 1 . 1 | 100 | 125 | 123 | $\frac{1}{42}$ | − |
| The same i[illegible] another, | tab. xii. fig. 4. | 1 . 1 | 100 | 125 | 120 | $\frac{1}{21}$ | − |
| | tab. iv. fig. 5. | 1 . 1 | 100 | 125 | 120 | $\frac{1}{22}$ | − |
| The same i[illegible] another, | tab xii. fig. 7. | 1 . 1 | 100 | 125 | 146 | $\frac{1}{6}$ | + |
| | tab. i. fig. 3. | 1 . 1 | 100 | 125 | 132 | $\frac{1}{21}$ | + |
| The same i[illegible] another, | tab. xii fig. 9. | 1 . 1 | 100 | 126 | 138 | $\frac{1}{11}$ | + |
| | tab. iii. fig. 1. | 1 . 1 | 100 | 126 | 136 | $\frac{1}{13}$ | + |
| The same i[illegible] another, | tab. xii. fig. 10. | 1 . 1 *q. p.* | 100 | [illegible] | 136 | $\frac{1}{13}$ | + |
| | tab. iii. fig. 3. | 1 . 1 *q. p.* | [illegible] | [illegible] | 123 | $\frac{1}{41}$ | − |
| The same i[illegible] another, | tab. xii. fig. 12. | 1 . 1 | 100 | [illegible] | 114 | $\frac{1}{11}$ | − |
| | tab. iii. fig. 3. | 1 . 1 | 100 | [illegible] | 110 | $\frac{1}{5}$ | − |
| The same i[illegible] another, tab. ii. fig. 1. | | 1 . 1 | 100 | [illegible] | 131 | $\frac{1}{25}$ | + |
| The same [illegible] another, tab. ii. fig. 2. | | 1 . 1 | 100 | [illegible] | 138 | $\frac{1}{11}$ | + |
| The same [illegible] another, tab. ii. fig. 3. | | 1 . 1 | 100 | [illegible] | 133 | $\frac{1}{18}$ | + |
| The same i[illegible] another, tab. xxv. | | 1 . 1 | 100 | [illegible]6 | 121 | $\frac{1}{25}$ | − |
| The same i[illegible] woman, tab. xiii. | | 1 . 1 | 100 | [illegible]26 | 124 | $\frac{1}{63}$ | + |
| From *RUYSCH.* | | | | | | | |
| The right [illegible]bclavian branched into the axillary and carotid, Ep. [illegible]obl. III. tab. iii. f. 2. | | 2 . 1 | 100 | 13[illegible] | 136 | $\frac{1}{16}$ | + |

Place thi[s] and t[he] following tables fronting page 144. vol. 3.

| The [d]ivaricated ARTERIES. | Proportions of the Branches. | Capacities of the Trunks. | Conjunct capacities of all the Branches. By the Theory. | By meaſuring. | Differences. | |
|---|---|---|---|---|---|---|
| The ſuperior branc[illegible] of the ſplenic artery entring the ſpleen, Ep probl. iv. ta[illegible]. iv. fig. 2. | 4 · 4 · 1 | 100 | 13[illegible] | [illegible]5 | $\frac{1}{12}$ | − |
| Its inferior branch, ibid. | 3 · 2 | 100 | 12[illegible] | [illegible]1 | $\frac{1}{31}$ | [illegible] |
| The uppermoſt art[illegible]ry from this inferior branch, ib. | 7 · 6 | 100 | 12[illegible] | [illegible] | $\frac{1}{31}$ | + |
| The lower one, ib | 5 · 4 | 100 | 12[illegible] | [illegible] | $\frac{1}{9}$ | + |
| A meſenteric arter[illegible], Muſ. anat. p. 76. fig. 5. | 1 . 1 q. p. | 100 | 12[illegible] | [illegible] | $\frac{1}{63}$ | ÷ |
| Its right branch, il[illegible] | 10 · 9 q. p. | 100 | 12[illegible] | [illegible] | $\frac{1}{13}$ | + |
| Its left branch, ib. | 5 · 1 | 100 | 113 | [illegible] | $\frac{1}{23}$ | − |
| From *KEILL.* | | | | | | |
| The ſuperior meſ[illegible]nteric artery, ſpending itſelf in twenty one branches, te[illegible]t. iv p. 88. | 2136, &c. | 100 | 258 | | $\frac{1}{23}$ | − |
| The 5th branch o[illegible] the meſenteric, ib. p 90. l 15. | 5 · 8 | 100 | 124 | | $\frac{1}{6}$ | − |
| The larger branch of this 5th meſenteric, ib. l 25. | 5 · 9 · 8 | 100 | 142 | | $\frac{1}{18}$ | ÷ |
| The 3d twig of th[illegible]is larger branch, p 91. l 4. | 4 · 3 q. p. | 100 | 125 | | $\frac{1}{5}$ | + |
| The 1ſt branch [illegible] t[illegible]e 8th meſenteric, ib. l 12. | 1 . 1 | 100 | 126 | | $\frac{1}{16}$ | − |
| The 2d branch [illegible] e 8th meſenteric. ib. l 16. | 4 · 5 | 100 | 126 | | $\frac{1}{18}$ | + |
| The 10th meſente[illegible]c art[illegible]y, ib. l 20. | 6 · 5 | 100 | [illegible]6 | | $\frac{1}{14}$ | − |
| The firſt branch [illegible]e 10th meſenteric, ib. l 24. | 5 · 6 | 100 | 126 | | $\frac{1}{42}$ | − |
| The 2d twig of th[illegible] 1ſt branch, p. 92. l 3. | 1 . 1 | 100 | 126 | | $\frac{1}{6}$ | − |
| The 14th meſente[illegible] ib. l 7. | 1 . 1 . 1 | 100 | 146 | | $\frac{1}{75}$ | − |
| The 15th meſente[illegible], ib. l 12. | 4 · 5 | 100 | 126 | | $\frac{1}{16}$ | + |
| The 2d branch of [illegible]is 15th meſenteric, ib. l 16. | 7 · 6 · 6 | 100 | 144 | | $\frac{1}{1[illegible]}$ | + |
| One of the twigs o[illegible] this ſecond branch, ib. l 21. | 3 · 4 | 100 | 125 | | $\frac{1}{20}$ | + |
| The femoral arter[illegible] dividing into two branches, Tentam. II. p 45. l 2. | 5 · 4 q. p. | 100 | 126 | | $\frac{1}{4}$ | + |
| Its 2d branch, ib. [illegible]3. | 3 · 5 q. p. | 100 | 124 | | $\frac{1}{11}$ | − |
| The 2d branch in [illegible]3. ib. l 4. | 4 · 2 | 100 | 135 | | $\frac{1}{6}$ | + |
| The [illegible]ſt branch in [illegible]4. ib. l 5. | 2 . 1 | 100 | 123 | | $\frac{1}{10}$ | + |
| The 2d branch in [illegible]5. ib. l. 6. | 11 · 5 | 100 | 122 | | $\frac{1}{20}$ | + |

| The divaricated ARTERIES. | Proportions of the Branches. | Capacities of the Trunks. | Conjunct Capacities of all the Branches. By the Theory. | Conjunct Capacities of all the Branches. By measuring. | Differences. | |
|---|---|---|---|---|---|---|
| The 1st branch i[illegible] l 6. ib. l 7. | 5 · 3 · 1 q. p. | 100 | 135 | 114 | $\frac{1}{6}$ | — |
| The 1st branch i[illegible] l 7. ib. l 8. | 3 · 2 | 100 | 124 | 124 | 0 | |
| The 1st branch i[illegible] l 1. ib. l 9. | 2 · 9 | 100 | 115 | 107 | $\frac{1}{14}$ | — |
| A branch of the [illegible]emoral artery in its progrefs, ib. l 10. | 3 · 2 · 30 q. p. | 100 | 109 | 113 | $\frac{1}{28}$ | + |
| The 3d branch i[illegible] l 10. ib. l 11. | 1 · 5 | 100 | 113 | 112 | $\frac{1}{113}$ | + |
| The 1st branch i[illegible] l 13. ib. l 14. | 2 · 1 | 100 | 123 | 144 | $\frac{1}{6}$ | + |
| The 1st branch i[illegible] l 14. ib. l 15. | 5 · 6 | 100 | 126 | 120 | $\frac{1}{21}$ | — |
| The 2d branch i[illegible] l 9. ib l 16. | 1 · 8 q. p. | 100 | [illegible] | 87 | $\frac{1}{5}$ | — |
| The 2d branch [illegible] l 16. ib. l 17. | 1 · 1 · 4 | 100 | 1[illegible]9 | 149 | $\frac{1}{7}$ | + |
| The 3d branch [illegible] l 17. ib. l 18. | 3 · 2 · 30 | 100 | [illegible]9 | 109 | 0 | |
| The 2d branch [illegible] l 5. ib. l 19. | 10 · 9 | 100 | [illegible]6 | 122 | $\frac{1}{31}$ | — |
| [illegible]rom my own Diffections. | | | | | | |
| The right fubcl[illegible]ian branched into the axillary and carotid, in a man, | 3 · 2 | 100 | 1[illegible]5 | 111 | $\frac{1}{9}$ | — |
| The fame in a [illegible]an, | 4 · 3 | 100 | [illegible] | 124 | $\frac{1}{125}$ | — |
| The fame in [illegible]g girl, | 8 · 9 | 100 | [illegible] | 119 | $\frac{1}{18}$ | — |
| The right c[illegible]rot[illegible] divided into the internal and external in a man, | 3 · 2 | 100 | 1[illegible]5 | 127 | $\frac{1}{53}$ | + |
| The fame in a [illegible] | 7 · 6 | 100 | 1[illegible]6 | 107 | $\frac{1}{7}$ | — |
| The aorta end[illegible] [illegible]n the iliacs in a boy, | 1 · 1 | 100 | 1[illegible]6 | 106 | $\frac{1}{6}$ | — |
| The iliac arter[illegible] branched into the external and internal in a man, | 5 · 4 | 100 | 1[illegible]6 | 128 | $\frac{1}{63}$ | + |
| The fame in a [illegible]oung man, | 5 · 4 | 100 | 126 | 130 | $\frac{1}{32}$ | + |
| The fame in a[illegible]y, | 5 · 4 | 100 | 1[illegible]6 | 118 | $\frac{1}{21}$ | — |
| The fame in an[illegible]her boy, | 5 · 4 | 100 | 1[illegible]6 | 130 | $\frac{1}{32}$ | + |
| The fame in an[illegible]her boy, | 10 · 9 | 100 | 1[illegible]6 | 129 | $\frac{1}{42}$ | + |
| The fame in a[illegible]oman, | 1 · 1 | 100 | 1[illegible]6 | 124 | $\frac{1}{63}$ | — |
| The fame in a[illegible]oung girl, | 5 · 4 q. p. | 100 | 126 | 132 | $\frac{1}{31}$ | + |
| [illegible] fame i[illegible] a[illegible]other young girl, | 5 · 4 | 100 | 126 | 131 | $\frac{1}{35}$ | + |

| The divaricated ARTERIES. | Proportions of the Branches. | Capacities of the Trunks. | Conjunct capacities of all the Branches. By the Theory. | By meaſuring. | Differences. |
|---|---|---|---|---|---|
| *...NICHOLS.* | | | | | |
| An artery A (wh... e to be one of the meſenterics) ramified into the ... es B, C, Comp. anat. tab. ii. p. 2. | 3 · 4 | 100 | 125 | 113 | $\frac{1}{1[illegible]}$ — |
| The branch C, d... nto D, E, - | 1 . 2 | 100 | 123 | 116 | $\frac{1}{17}$ — |
| The branch E di... to the ſmaller branch 36, and another (Z) whic... to be about 21, - | 5 · 3 | 100 | 124 | 116 | $\frac{1}{15}$ — |
| The branch (Z) r... into the twigs 16 and 9, | 5 · 3 *q. p.* | 100 | 124 | 119 | $\frac{1}{25}$ — |
| *...lany Obſervations.* | | | | | |
| A branch A of th... nal carotid, divided into the anterior and poſteri... ches C, B, *Du Verney Org. de l'Ouie*, tab. ii. fig - - | 5 · 3 *q. p.* | 100 | 124 | 100 | $\frac{1}{5}$ — |
| The ſame, ib. fig. - | 4 · 3 *q. p.* | 100 | 125 | 13 | $\frac{1}{10}$ — |
| The anterior bran... ter throwing off D, ſubdivided into two branches, ... g. 1. - | 2 . 1 *q. p.* | 100 | 123 | 33 | $\frac{1}{12}$ + |
| The deſcendin... ding in the inferior meſenteric, Lumbars and ilia... *erheyen*, anat. tab. xvi. fig. 1. | 3 . 1 . 1 . 40<br>40 *q. p.* | 100 | 132 | 34 | $\frac{1}{66}$ + |
| The aorta endi... iliacs, the inferior meſenteric, 4 Lumbars and ... e, Cowper anat. hum. Bod. app. tab. iii. - - | 23 . 26 . 2<br>1 . 1, &c. | 100 | 140 | 2[illegible] | $\frac{1}{9}$ — |
| The right iliac into ... xternal and internal, ib. | 1 . 1 | 100 | 126 | 06 | $\frac{1}{6}$ — |
| The left iliac divide... ſame way, ib. - | 1 . 1 | 100 | 126 | [illegible] | $\frac{1}{[illegible]}$ [illegible] |
| The cæliac into 2 br... es, Cheſelden anat. tab. xvii. 2. | 3 · 2 *q. p.* | 100 | 125 | | |
| The larger branch o... cæliac into leſſer branches, ib. | 9 · 9 · 5 | 100 | 142 | | |
| The ſuperior meſent... divided into three branches, ib. 3. | 5 · 6 · 1 | 100 | 129 | | |
| The inferior meſent... divided into three branches, ib. 4. | 11 . 20 . 11 | 100 | 121 | | |
| The deſcending aor... ding in the iliacs, ib. tab. xv. | 1 . 1 | 100 | 126 | | |
| The cæliac artery b... ing into the right and left Stukeley of the ſpleen, ... i. - | 9 . 10 | 100 | 126 | | |
| A capillary artery o... inteſtines ſending off eight equal branches, Hales ... aſtat. ix. 14. p. 53. | 1 . 1 . 1 . 1<br>1 . 1 . 1 . 1 | 100 | 200 | | |

multocular, having, in effect, as many eyes as there are perforations in the cornea. By which means, as other creatures, but with two eyes, are obliged, by the contraction of the muscles above described, to turn their eyes to objects, these have some or other of their pupils always ready placed towards objects nearly all round them; whence they are so far from being de-[...]d any benefit of this noble and most necessary sense of sight, that they have probably more of it than other creatures, answering to their necessities and way of living. And thus provident nature has with great industry and art provided for the immobility of the head and eyes.

II. As in man and most other creatures, the eyes are situated in the head, because, amongst other reasons, it is the most convenient place of their defence and security, being composed of hard bones, wherein are formed two large strong sinuses or sockets, commonly called orbits, for the convenient lodging of these tender organs and securing them against external injuries; so in those creatures, whose head, like their eyes and the rest of their body, is soft and without bones, nature hath provided for this necessary and tender organ [...]nd of guard, by enduing the [...]aculty of withdrawing its eyes [...]nd lodging them in the same [...]body. We have a very beauti-[...] this in snails, whose eyes are [...] four horns, like atramentous [...]e end of each horn, which [...] at pleasure when in any dan-ger.

The aorta ...
The iliac arter[...]
a man,
The same in a
The same in a
The same in an
The same in a
The same in a
The same in a
The same in a

ger. I know the learned Perrault (in his *mechanique des animaux*) feems to doubt of fnails having eyes: And Dr Brown ranks this conceit of the eyes of fnails amongft the vulgar errors of the multitude; but a good microfcope would foon have fhewn him his own error. Thofe that defire further fatisfaction in this particular, may confult Dr Power's obfervations, and *Lifter de Cochleis & Limacibus.*

If it fhould be here afked, Whence it is that fifhes, whofe eyes are not guarded and defended by eye-lids, fhould not alfo have a power of retracting their eyes for their defence and fecurity? To which I anfwer, That, if we reflect on the hardnefs of the cornea, which, in all animals that want eye-lids, exactly refembles the horn of a lanthron, and therefore is not to be hurt by fuch particles as their eyes are commonly expofed to, we muft fee that fuch a mechanifm would have been ufelefs: And befide, in fome cruftaceous animals, whofe occafions and manner of living, perhaps, expofe their eyes to greater dangers and inconveniencies, their eyes are well fecured by deep finufes, into which, as into a fafe chamber, they can retract their eyes upon the approach of any danger, as has been well obferved by *Fabricius ab Aquapendente* (in his treatife *de Oculo, cap.* 14).

Something of a mechanifm fimilar to this has alfo been thought to obtain in the eyes of moles, which are not blind, as Ariftotle, Pliny, Severinus, &c. would perfuade us; but, being provided with little black eyes, about the bignefs of a fmall pin-head, in which not only the

the aqueous, vitreous, and cryſtalline humors, but alſo the *ligamentum ciliare*, copped or conical cornea, with the round pupil and optic nerve, have been manifeſtly diſcerned, they muſt neceſſarily ſerve to guide and ſecure it, when it chances to be above ground. But, becauſe this animal lives moſt under ground, which it digs and penetrates, it was neceſſary their eyes ſhould be well guarded and defended againſt the many dangers and inconveniencies to which their manner of living expoſes them; and this is the reaſon why their eyes are ſo ſmall, and that they are ſituated ſo far in the head, and covered ſo ſtrongly with hair, that they can be of no ſervice to them, unleſs they be poſſeſſed of a power of protruding and retracting them at pleaſure, more or leſs as they have more or leſs occaſion to uſe or guard their eyes, as has been obſerved by Borrichius, *Epiſt. Bartholin.* 92. *cent.* iv. Mr Derham's Phyſico-theology, book iv. chap. 2. &c.

III. The third and laſt reflection we ſhall make upon the motion of our eyes, is what regards a problem which has very much perplexed both phyſicians and philoſophers, *viz.* What is the cauſe of the uniform motion of both eyes?

In ſome creatures, ſuch as fiſhes, birds; and, among quadrupeds, the hare, camelion, *&c.* the eyes are moved differently, the one towards one object, and the other towards another: But in man, ſheep, oxen, and dogs, the motions are ſo uniform, that they never fail to turn both towards the ſame place. Hence, in operations upon the eye, that require it to be kept immove-

able for ſome time, it is neceſſary to tie up the ſound eye with compreſs and bandage, by which means the other is eaſier kept fixed and immoveable.

The final cauſe of this uniform motion of our eyes is,

1. That the ſight may be thence rendered more ſtrong and perfect; for ſince each eye apart impreſſes the mind with an idea of the ſame object, the impreſſion muſt be more ſtrong and lively when both eyes concur, than when only one; and conſequently the mind muſt receive a more ſtrong, lively, and perfect idea of the object in view, as is agreeable to experience: And that both may concur, it is neceſſary they move uniformly; for, though the retina, or immediate organ of viſion, be expanded upon the whole bottom of the eye as far as the *ligamentum ciliare*, yet nothing is diſtinctly and clearly ſeen but what the eye is directed to. Thus, in viewing any word, ſuch as MEDICINE, if the eye be directed to the firſt letter M, and keep itſelf fixed thereon for obſerving it accurately, the other letters will not then appear clear or diſtinct, becauſe the ſeveral pencils of rays that come therefrom fall too obliquely on the cryſtalline and other humors of the eye, to be accurately collected in ſo many diſtinct points of the retina; and chiefly becauſe of a certain degree of hardneſs, calloſity, or inſenſibility, that obtains in all parts of the retina, excepting towards the axis of the eye, directly oppoſite to the pupil. Hence it is, that to view any object, and thence to receive the ſtrongeſt and moſt lively impreſſions, it

it is always neceſſary we turn our eyes directly towards it, that its picture may fall preciſely upon this moſt delicate and ſenſible part of the organ, which is naturally in the axis of the eye. But if this moſt ſenſible and delicate part happen, from a fault in the firſt conformation, or from any other cauſe, not to be in the optic axis, but a little off at a ſide, then, to ſee an object clearly, the eye muſt not be directed towards it, but a little to a ſide, that its picture may fall on this moſt ſenſible part of the organ: And this may be one cauſe of ſquinting, which, as is eaſy to ſee, muſt be altogether incurable.

Now, though it is certain that only a very ſmall part of any object can at once be clearly and diſtinctly ſeen, namely, that whoſe image on the retina is in the axis of the eye; and that the other parts of the object, which have their images painted at ſome diſtance from this ſame axis, are but faintly and obſcurely perceived, yet we are ſeldom ſenſible of this defect; and, in viewing any large body, we are ready to imagine that we ſee at the ſame time all its parts equally diſtinct and clear: But this is a vulgar error, and we are led into it from the quick and almoſt continual motion of the eye, whereby it is ſucceſſively directed towards all the parts of the object in an inſtant of time; for it is certain, that the ideas of objects, which we receive by ſight, do not preſently periſh, but are of a laſting nature, as appears from what happens when a coal of fire is nimbly moved about in the circumference of a circle, which makes the whole cir-

cumference appear like a circle of fire, because the idea of the coal, excited in the mind by the rays of light, are of a lasting nature, and continue till the coal of fire, in going round, return to its former place; and therefore, if our eye takes no longer time to direct itself successively to all the small parts of an object, than what the coal of fire takes to go round, the mind will distinctly perceive all those parts, without being sensible of any defect or insensibility in any part of the retina, because the idea of one part continues, till, by the motion of the eye, the image of the other parts be successively received upon the same most sensible part of the retina: And this is the reason why the globe of the eye moves so quickly, and that its muscles have such a quantity of nerves to perform their motions. But I go on.

2. A *second* advantage we reap from the uniform motions of our eyes, which is yet more considerable than the former, consists in our being thereby enabled to judge with more certainty of the distance of objects.

There are six means which concur for our judging of the distance of objects, of all which the most universal, and frequently the most sure, is the angle which the rays of light make at the object in coming thence to our eyes. When this angle is very great, we see the object very near; and, on the contrary, when it is very small, we see it at a great distance; and the change which happens in the situation of our eyes, according to the change of this angle, is a mean which our mind makes

makes ufe of for judging of the diftance and proximity of objects. To be perfuaded of the truth of this, fufpend by a thread a ring, fo as its fide may be towards you, and its hole look right and left, and taking a fmall rod, crooked at the end, in your hand, retire from the ring two or three paces, and having with one hand covered one of your eyes, endeavour with the other to pafs the crooked end of your rod through the ring. This appears very eafy, and yet, upon trial, perhaps once in a hundred times you fhall not fucceed, efpecially if you move the rod a little quickly. This furprifing difficulty, which is found in paffing the rod, arifes, becaufe, when one eye is fhut, the angle which the rays of light make at the object, in coming thence to both eyes, is not known; for, in any triangle to know the bignefs of an angle, it is not fufficient to know the length of the bafe fubtending that angle, and the magnitude of the angle which one of its fides make with that bafe, as is known to mathematicians, but it is alfo neceffary to know the other angle which the other fide makes with the bafe: But this can never be known but in opening both eyes, and directing them to the object; and therefore the mind can never make ufe of its natural geometry, for judging of the diftance of the ring, when one of the eyes is fhut.

From this we may fee the ufe of having two eyes placed at a certain diftance from one another; for, by ufe, we get a habit of judging of the diftance of objects by the direction of the axes, which is fenfible to us, becaufe it depends

pends on the motion of the eye that we feel. But other creatures that look differently with their eyes, as fishes, fowls, the hare, camelion, &c. cannot judge of the distance of objects from this angle, and therefore must be more liable to mistakes than we are; yet nature has provided them with two eyes, that their sight might not be too much limited, but that they might see objects equally well on both sides, and thereby be better enabled to seek their food, and avoid dangers: Whence it is, that in some animals they are seated so as to see behind them, as well as on each side. We have a very remarkable example of this in hares and conies, whose eyes are very protuberant, and placed so much towards the sides of their head, that their two eyes take in nearly a whole sphere; whereas, in dogs, that pursue them, the eyes are set more forward in the head to look that way more than backward.

From this also we may see, why we err so frequently in the judgments we form of the magnitude of objects seen only with one eye: For, since we judge not of the extension or magnitude from the apparent magnitude alone, but also from the apparent distance, it follows, that objects seen with one eye must appear smaller or greater, as they are imagined nearer or further off. Thus, a planet viewed with a telescope, sometimes is judged near the eye-glass; and therefore appears very small, while to others it appears very great, because imagined a good way beyond the objective. The same thing happens in viewing one's self in a great concave mirror not too far off; when the one

one eye is ſhut, the face does not appear very big, becauſe it is imagined at no greater diſtance than the ſurface of the mirror; but to both eyes it appears a great deal bigger, becauſe it is then imagined much farther off, as has been obſerved by Mr Mariotte (*Traite des couleurs*).

It being therefore manifeſt, that the diſpoſition of our eyes, which always accompanies the angle formed of the viſual rays that flow to both pupils, and that cut one another in that point of the object on which our eyes are fixed, is one of the beſt and moſt univerſal means we have for judging of the diſtance of objects; it needs be no ſurpriſe, that in very great diſtances, where the diſtance of our eyes bears no ſenſible proportion to the diſtance of the object, it ſhould be impoſſible for us, by this or any other method, to judge rightly of the diſtance, becauſe the change that happens here to this angle is ſo ſmall, as to be altogether inſenſible.

Every body [illegible] angle changes conſiderably, when an object that is only a foot from our eyes, is tranſported to four; but if from four it be tranſported to eight, the change is by much leſs ſenſible; if from eight to twelve, it is yet leſs; if from a thouſand to an hundred thouſand, it is ſcarce any more ſenſible, nay, not though the diſtance be increaſed from a thouſand to an infinite ſpace.

It is for this reaſon that we are ſo often deceived in the judgment we form of all great diſtances, and that we ſee the ſun, moon, and ſtars, as if they were involved in the clouds, though

though it is certain they are vaſtly beyond them. And, being deceived as to their diſtance, we muſt alſo be deceived with reſpect to their magnitude. Thus the moon ſeems greater than the greateſt ſtar, though every body knows that ſhe is vaſtly leſs. Thus the ſun and moon appear not above a foot or two in diameter, if we truſt the teſtimony of our eyes, as did Epicurus and Lucretius, who therefore imagined them no bigger than what they appeared. Thus alſo the ſun and moon appear greater when near the horizon, than at a greater height, becauſe, when nigh the horizon, they are judged at a greater diſtance.

There is yet another advantage full as conſiderable as any of the former, that is thought to ariſe from the uniform motion of our eyes, and that is, the ſingle appearance of objects ſeen with both eyes.

This indeed, at firſt view, does appear very probable; for if, in looking to any object, you preſs one of your eyes aſide with your finger, and alter its direction, every thing will be ſeen double, which is a common experiment where with children amuſe themſelves, being delighted with the uncommon double appearance of objects.

The ſame thing alſo happens, when either of the eyes is, from a ſpaſm or paralyſis of any of its muſcles, or from any other cauſe, reſtrained from following the motions of the other. Thus Willis (in his *Anima brutorum*, cap. 15.) tells us of a young man, long ill of the palſy, who at laſt came out to ſee all things double, from a ſpaſm in the adducent muſcle of his left eye,

eye, whereby its axis was turned inwards, ſo that it could not be directed to the ſame object with the other.

Platerus likewiſe (in the firſt book of his Obſervations, p. 132.) gives us the hiſtory of a boy, who, after having receiving a ſtroke on his head, became paralytic in one of his ſides, and had his mouth diſtorted; to whom every thing he looked at appeared double: And tho' he does not attempt to account for this depravation of ſight, yet it is eaſy to ſee that it could proceed from nothing but a palſy or ſpaſm of one of the muſcles of one of his eyes, by which it was rendered incapable of following the motion of the other.

Langius alſo has a very remarkable caſe to this purpoſe, which, being a little uncommon, we muſt not omit. He tells us, (in the 7th Epiſtle of his firſt book), That, in a wound of the eye, it happened, through neglect, to unite and adhere to the under eye-lid; ſo that, after the cure, that eye was tied down, and rendered incapable of following the motions of the other: This occaſioned every thing to appear double, till the eye, by its frequent motions, had at laſt ſtretched the eye-lid, to which it was adherent, and thereby recovered its former liberty of moving uniformly with the other.

Multitudes of caſes of this kind might be advanced, but I like not, without neceſſity, to multiply examples of the ſame nature; theſe are ſufficient to prove, that, when our eyes are reſtrained from moving uniformly, all objects are ſeen double. Neither is it to be doubted, but

but, when the ſame phænomenon occurs in drunk or maniac perſons, it proceeds from the like cauſe: The uniform motion of our eyes requiring an eaſy and regular motion of the ſpirits, which frequently is wanting in ſuch caſes.

The ſame thing does alſo happen ſometimes ſoon before death, when the ſpirits have been worn out and exhauſted by long ſickneſs. We have a remarkable example of this in the *Acta Hafnienſia*, publiſhed by Bartholin. Olaus Borrichius, there tells us (vol. 2. p. 198.) of a woman that had been long ill of a diſeaſe in her breaſt and ſpleen, to whom, two days before her death, all things appeared double. He indeed attributes this phænomenon to a change in the figure of the humors of the eye, and thinks that they had acquired the form of a polygon, or multiplying glaſs; which is a very ſtrange out of the way notion, and altogether improbable. The true cauſe thereof ſeems to have ariſen from the languid irregular motion of the animal ſpirits diſqualifying them from executing the commands of the will, and directing both eyes to the ſame object.

For theſe, and ſuch like, reaſons, it is, that very many, both phyſicians and philoſophers, have been brought to believe, that, to ſee objects ſingle, it is abſolutely neceſſary that both eyes be directed to the ſame object, and that this is one of the final cauſes of their uniform motion; and yet, when the matter ſhall be duly examined, I am confident, little foundation will be found for any ſuch conſequence. But

I

I must delay entering upon this subject, till I have assigned what to me appears to be the true reason of this phænomenon; because a principle or two will be there established, necessary to be carried along in our thoughts throughout the whole of this argument.

Why objects seen with both eyes do not appear double, is a problem that has employed the genius and invention of the greatest men of all ages. Gassendus and Porta found such difficulty in reconciling this appearance with the ordinary principles of philosophy and optics, that they have been forced to suppose, that, though both eyes are open, yet we only see with one at a time. But this, being so obviously contrary to common experience, serves more as an example to shew what surprising lengths fancy may carry even the greatest men, than to satisfy the curious.

Galen imagined that this single appearance of objects proceeds from the close coalition of the optic nerves behind the *os sphenoides*, and seems to triumph in the discovery, as if he had found out a fine reason why our optic nerves are thus united, (*lib.* 10. *de offic. part. cap.* 14.) In this he is followed by a great many philosophers as well as physicians, though they are much divided among themselves in their manner of explaining it. Galen himself had recourse to a communication of pores. and followed the doctrine of Herophilus in giving to each nerve one pore, which he made to communicate at the conjunction of the nerves. But as this communication of pores is, by our best anatomists, now looked on as mere hypo-

thesis, that has no foundation in nature, so it is altogether insufficient for solving the problem, though, in place of one pore, each nerve should, agreeable to the more modern notion, be allowed to have as many of them as there are nervous fibres, as some of the followers of Galen have indeed supposed.

Our justly eminent Sir Isaac Newton (in the queries annexed to his optics) accounts for it thus: "Are not the species of objects seen "with both eyes united, where the optic "nerves meet before they come into the "brain? the fibres on the right side of both "nerves uniting there, and, after union, going "thence into the brain, in the nerve which is "on the right side of the head; and the fibres "on the left side of both nerves uniting in "the same place, and, after union, going "into the brain, in the nerve which is on "the left side of the head; and these two "nerves meeting in the brain, in such a man-"ner that their fibres make but one entire "species or picture, half of which, on the "right side of the sensorium, comes from "the right side of both eyes, through the "right side of both optic nerves, to the "place where the nerves meet, and from "thence on the right side of the head, into the "brain; and the other half, on the left side of "the sensorium, comes in like manner from "the left side of both eyes."

This is indeed the most beautiful and ingenious explication of the manner how an object appears single from the coalition of the optic nerves that ever appeared; and to render

der it ſtill more probable, the ſame great man obſerves, "That the optic nerves of ſuch ani-"mals as look the ſame way with both eyes "(as of men, ſheep, dogs, oxen, &c) meet "before they come into the brain; but the op-"tic nerves of ſuch animals as do not look the "ſame way with both eyes (as of fiſhes, and of "the chamelion) do not meet." Theſe reaſons do indeed render his hypotheſis very probable; yet there are others ſo demonſtrative of the contrary, that it is not to be doubted, but, had they been known to Newton, he had retracted his opinion, eſpecially ſince the thing admits of an eaſy ſolution, without any ſuch ſuppoſition: For,

Although the optic nerves are united at the *ſella turcica*, yet this happens without any confuſion or decuſſation of their fibres. It is indeed true, that their conjunction is ſo cloſe, that their ſubſtances ſeem to be confounded, yet there are ſeveral obſervations which prove that they are united only by a cloſe conjunction, without any decuſſation, interſection, mixture, or confuſion of ſubſtance, of which I ſhall only mention two; the one is from the accurate and faithful anatomiſt Veſalius. His words are: "Porro, iis qui de con-"greſſu hoc meatuque acriter citra partium in-"ſpectionem indies altercantur, non gravabor "duo, quae in congreſſu animadverti, hic adji-"cere, quo et hinc ſuarum nugarum habeant "argumenta. Patavii itaque adoleſcens ſuſpen-"dio necatus, publicae ſectioni adhibitus fuit, "cui ante annum dexter oculus a carnifice erutus "fuerat: Deinde mulier, eodem ſupplicio af-"fecta, nobis obtigit, cui dexter quoque oculus

"ab ineunte aetate emarcuerat, ſiniſtro interim "integerrimo. Mulieri dexter nervus toto progreſſu longe tenuior ſiniſtro viſebatur, non ſolum extra calvariae cavitatem, verum in exortu "quoque, et in dextra congreſſus nervorum ſede. Ac praeterquam quod dexter tenuis erat, durior quoque et rubicundior cernebatur, "uti ſane et in adoleſcente; ſed dexter non admodum, neque craſſitie, neque mollitie adhuc "ſiniſtro cedebat." (*De corp. hum. fabrica*, lib. iv. c. 4.) The other obſervation which I ſhall take notice of, is from Cæſalpinus, whoſe words, as recorded by Riolan, (Anthropograph. lib. iv. cap. 2.) and Diemerbroeck (*Anatom. corp. human.* lib. iii. cap. 8.) are as follows, "Repertus eſt aliquando in anatome alter ex "nervis viſoriis attenuatus, alter plenus: Viſus "autem erat imbecillis in oculo ad quem nervus extenuatus ferebatur; habuit enim vulnus in capite circa eandem partem; nervus "autem extenuatus non ad oppoſitam partem "procedebat, ſed ad eandem flectebatur. Viſum "hoc eſt Piſis anno 1590. Unde omnes ſpectatores argumentum id certum exiſtimaverunt, nervos viſorios nequaquam ſe interſecare, "ſed coire et regredi ad eandem partem."

From theſe and ſuch like obſervations, it plainly follows, That our optic nerves do not interſect each other, nor mix and confound their ſubſtances, but are only united by a cloſe coheſion, contrary to what the opinion of Newton and the Galeniſts ſuppoſes.

But this is not all; for, ſuppoſing our optic nerves to be united in what manner they pleaſe; yet that the ſingle appearance of objects ſeen with

with both eyes, does not depend on this union, ſeems evident from another obſervation of the ſame Veſalius, which being a little uncommon, I ſhall alſo ſet down at length in the author's own words. "His ille acceſſit" (ſays he, ſpeaking of the former obſervations) "cujus nervos viſorios illo de quo hic ſermo eſt "congreſſu invicem non connaſci, neque ſeſe con-"tingere, vidimus; ſed dexter nonnihil eâ ſede, "quâ calvariam egreſſurus fuerat, ſiniſtrorſum, "et ſiniſter nonnihil dextrorſum, reflectebatur, "quaſi non coalitûs occaſione nervi congrederen-"tur, verum ut commode per ſuum foramen è "calvaria prociderent; potiſſimum quum etiam "hoc ductu progredientes, in oculi poſterio-"ris ſedis medium non inſerantur. Quam ſe-"dulo autem ac ſollicite ejus viri, cui in eum "modum nervi dehiſcebant, familiares, num "illi omnia gemina perpetuo oculis obverſa-"rentur, interrogaverimus, neminem naturæ "operum cognitione flagrantem ambigere ſat "ſcio; at nihil aliud reſciſcere licuit, quam "ipſum de viſu nunquam conqueſtum fuiſſe, vi-"ſuque præſtante ſemper valuiſſe, familiareſque "de viſorum duplicatione nihil unquam intellix-"iſſe." Seeing then that objects have appeared ſingle to ſuch as had their optic nerves disjoined, it muſt be allowed that this phænomenon depends on ſomething elſe than the coaleſcence or decuſſation of theſe nerves. Others therefore, laying aſide all the hypotheſes that are founded upon any coaleſcence, contact, or croſſing of the optic nerves, have ſought for the cauſe of this phænomenon in a certain ſympathy betwixt them. To explain this ſympathy, Mr Rohault (*Phyſic. part.* 1. *cap.* 31.) ſuppoſes, that in each nerve

there are juſt as many fibres as in the other, and that the correſponding fibres of both nerves are united in the ſame point in the ſenſorium, *e.g.* Suppoſe, as in Tab. I. Fig. 1. the nerves compoſed of five fibres, whoſe extremities in the right eye are A, B, C, D, E, and in the other eye, *a*, *b*, *c* *d*, *e*. The correſponding fibres, A*a*, B*b*, C*c*, D*d*, and E*e*, are ſuppoſed to meet in the ſenſorium S, in the points α, β, κ, δ, ε. Hence, if both eyes are directed to F, its image will fall on the retina at the optic axes, and there ſtrike the ſympathiſing fibres C and *c*; which motion being propagated to the ſingle point of the ſenſorium κ, muſt there make but one ſpecies or picture. In like manner, the eyes retaining the ſame direction, the image of the point G will fall upon the right ſide of both eyes; and, by ſtriking the correſpondent fibres E and *e*, will, in the ſenſorium, make but one impreſſion at ε, where theſe fibres terminate; and the image of the point H, by ſtriking the correſponding fibres A and *a*, will, in the ſenſorium, make but one impreſſion at α: And thus, though both eyes receive the ſame impreſſions from objects, yet they are not ſeen double, becauſe, of theſe two impreſſions or images, one is only formed in the ſenſorium.

But neither has this hypotheſis, however ſpecious, any foundation in nature, for if, with Des Cartes, we ſuppoſe the *glandula pinealis* to be the ſenſorium, or chief ſeat of the ſoul, anatomy teaches us, that the nerves are not inſerted into it; and if, with Willis, we ſhould place the ſenſorium in the *corpora ſtriata*, or any other part of the brain, theſe being double and

and alike on both ſides, can never make one individual ſenſorium, in which all the correſponding analogous nervous fibres are united.

The judicious Dr Briggs (as may be ſeen at large in the Philoſophical Tranſactions, and in his *nova viſionis theoria*, annexed to his ophthalmographia) has invented another hypotheſis for explaining this ſuppoſed ſympathy of our eyes, without having recourſe to any meeting, communication, or conjunction of the nervous fibres in the ſenſorium. He ſuppoſes that the optic nerves conſiſt of homologous fibres, which have their riſe in the *thalami nervorum opticorum*, and are thence continued to both retinæ, and that theſe fibres have the ſame ſituation, diſpoſition, and tenſion in both eyes, *e. g.* he ſuppoſes that the fibres going to the upper part of the retinæ have a greater degree of tenſion; thoſe going to the under part, a ſmaller degree of tenſion; and thoſe going to the correſponding ſides, correſponding degrees of tenſion, and ſo forth; and conſequently, ſays he, when an image is painted on the correſponding parts of each retina, the ſame effects are produced, the ſame notice or information is carried to the thalamus, and ſo imparted to the ſoul, or judging faculty; for the homologous and correſponding fibres of both retinæ, upon which the image falls, having the ſame degree of tenſion, may be conceived as cords of two muſical inſtruments in concord and uniſon, which, from the impreſſions of light, are put into the ſame vibrations; ſo that the mind can have but one ſenſation from the ſame object,

ſince

ſince the two impreſſions are reunited in one, by the ſimilar and like diſpoſition of the fibres of the two nerves, which do ſo correſpond with each other, and which have ſuch a conformity and ſimilitude in their vibrations, that the ſoul cannot hinder itſelf from identifying the two impreſſions which it receives therefrom.

This is the ſubſtance of Dr Brigg's hypotheſis; to confirm which, he flies to experience and obſervation, pretending that this variety of tenſion in our nervous fibres, is owing to their greater or leſſer flexure in the thalami, which, he ſays, is manifeſt to the naked eye; and finding that the fibres on the inſide of both thalami agree in flexure, as alſo thoſe on the outſide, *&c.* he concludes that they agree alſo in tenſion, and conſequently ſympathize by a ſimilitude in their vibrations. But, unluckily for the author, this curious obſervation of his is ſo far from confirming his hypotheſis, that it quite undermines it; for all objects, a little to a ſide of the concourſe of the optic axes, would then appear double, by being painted on diſſimilar parts of the retinæ; whereas, had he laid aſide his obſervation, and with Newton, Rohault, *&c.* contented himſelf in ſuppoſing, that the fibres on the inſide of one eye ſympathize with the external fibres of the other, his hypotheſis had not been chargeable with this abſurdity, though even then many reaſons are not wanting for rejecting it: As,

1. It is a very difficult matter to conceive how the ſoft, tender, and delicate fibres of the

retina

ratina and medullary part of the optic nerves, can, without breaking, ſuffer that ſtrong tenſion which ſeems neceſſary to qualify them for being put into thoſe vibrating motions, in which he makes viſion to conſiſt. And it is more probable, that the impreſſions made upon our organs produce an undulation and refluctuation of the ſpirits, or of Newton's *materia ſubtilis* in the nervous fibrils, which reaching the ſenſorium, gives us the ideas of objects, than that theſe ideas ſhould be excited by theſe vibrations themſelves. This I might eaſily evince from a great many arguments, which I muſt not now mention, becauſe they would carry us too far out of our road: And eſpecially, becauſe, though we allow all ſenſation to proceed from the vibrations of our nervous fibres, yet this does not appear ſufficient to eſtabliſh his hypotheſis: For,

2. Suppoſing all ſenſation to proceed from vibrations excited in the nervous fibres, and that theſe fibres, in the analogous and correſponding parts of the retinæ, have the ſame degree of tenſion: *ex. gr.* Suppoſe the tenſion of the ſuperior fibres of both retinæ to be the ſame, as alſo that of the inferior; but that the fibres which terminate in the ſuperior part are more tenſe than thoſe that go to its inferior part: This is what our author ſuppoſes. Now, according to this hypotheſis, it would follow, that viſion would be more clear and ſtrong when cauſed by rays ſtriking the upper part of the retina, than when cauſed by rays ſtriking its inferior part, where the fibres being leſs ſtretched, muſt make the vibrations more languid and faint;

faint; and consequently an object placed below the optic axis, by painting its image upon the superior part of the retina, would appear more clear and lively than when it is placed above it: But this is contrary to all our experience; and what we have said of the superior and inferior fibres, holds equally in all the rest: For vision being every where uniform, it is necessary that the fibres, by whose vibration it is supposed to be occasioned, should be every where equally stretched.

3. If the concord, harmony, and equal tension of the analogous and corresponding nervous fibres, were the reason why objects seen with both eyes appear not double, it is not easy to conjecture, why this depravation of sight does not happen oftner that it is found to do in practice, and especially in such as abound with serous and phlegmatic humors, and whose brain is moist and lax, and who thereby are disposed to the *gutta serena*, or subject to convulsions and other nervous diseases; for, in such, the fibres of one of the optic nerves may easily happen to be relaxed, while the corresponding fibres of the other nerve still retain their natural tension; and, in particular, why did not that woman, formerly mentioned from Caesalpinus, see objects double, since one of the optic nerves was found withered and emaciated, while the other remained moist, plump, and juicy as before? Surely one would expect that this could not have happened in the nerves, without affecting the tension of their fibres, and thereby have occasioned objects to appear double; yet no such thing happened;

happened; for every thing appeared ſingle as before, only the ſight of the diſeaſed eye was more faint and obſcure than in the other: From which it follows, that the cauſe of this phænomenon is to be ſought for ſome where elſe, than in the ſimilar ſituation, diſpoſition, and tenſion of the nervous fibrillæ.

The true cauſe why objects appear not double, though ſeen with both eyes, to me ſeems wholly to depend on the faculty we have of ſeeing things in the place where they are. But, to explain and confirm this, I muſt premiſe the following

## LEMMA.

"Every point of an object appears and is ſeen "without the eye nearly in a ſtraight line, "drawn perpendicular to the retina, from "that point of it where its image falls."

That this is true, we may gather from many experiments. The following is very eaſy and convincing.

Suppoſe E the globe of the eye, furniſhed with all its coats and humors (See fig. 2. and 3.) and let A be a ſmall object, ſuch as the head of a pin, whoſe diſtance from the eye muſt be greater or leſs than that at which an object would be moſt diſtinctly ſeen when viewed with the naked eye. Cloſe to the eye place a card, or piece of opaque paper, in which is a ſmall hole made with a pin, and let QT repreſent the card. If this hole be ſuppoſed at *x*, then the ray of light A*xn*, falling on the retina at *n*, will there paint the image of the ſmall object A, and make it appear

pear in the visual line *nx*A, which is perpendicular to the retina at the point *n*. But, if the card be brought lower, so as its small hole may be at *r*, the ray of light A*r*, after passing the hole, will be refracted in the eye, so as to fall upon the retina at some other point, as *m*: For the object A being supposed at a greater or lesser distance than that at which an object may be most distinctly seen with the naked eye, all its rays that pass the pupil must be made to converge to a point, either before or behind the retina, such as *o*; but, on the retina itself they must fall on different points, according to the situation of the hole through which they pass; for the eye does not change its conformation, and adapt itself to the distance of an object viewed through a perforated card, as it always does when objects are viewed naked without any such interposition. Now the object A seen through the hole *r*, does not appear in its real place A, but at some other place, as B, viz. in the right line *m*B, which is perpendicular to the retina at the point *m*; and if the card be raised, so as the ray A*d* may pass the hole at *d*; after refraction, it will fall upon the retina at *i*, and the object will appear nearly at C in the perpendicular *i*C.

In like manner, if the card be pierced with three small holes, whose distance does not exceed the diameter of the pupil, as in *d*, *x*, and *r*, then the little object A will, at the same time, be seen in three different places, C, A, and B, and must therefore appear multiplied, according the number of holes; which evidently

evidently proves, that the rays that flow from the object through these holes, fall upon different points of the retina; and that there are three lines drawn perpendicularly from these points in the retina, in each of which the object is distinctly seen. We might here observe, that if the object A be brought to that precise distance from the eye that is necessary for uniting all its rays in one single point of the retina, as *n*, then it will appear single, though viewed through several holes: And the same thing must happen, though the middle hole be covered, so as no rays fall upon the eye but what pass at the holes *d* and *r*, towards the extremities of the pupil; for, these rays being united in the retina at *n*, the object will be seen in the visual line *nx*A, tho' no rays pass that way. From all which it is evident, that every little object, or point of an object, appears and is seen in the visual line, drawn perpendicular to the retina, at that point of it where its image falls.

To make this experiment with exactness, you must, for an object, look to a small luminous point in a dark place, such as a little hole in a card placed before a candle, or else you must look to a small black object placed on a white or luminous surface, as has been observed by Scheneir, (*Fundament. optic.*) who has several other curious enough observations relative to this experiment. Any trials I had occasion to make succeed well enough with a pin placed before a well lighted window.

It is from this principle, that, when a man in the dark rubs the under part of either cor-

ner of his eye with his finger, and turns his eye away from his finger, he will see, towards the opposite side of the eye, a circle of firey colours like those in the feather of a peacock's tail, which can arise from nothing but such motions excited in the retina by the pressure and motion of the finger, as at other times are excited there by light coming from the opposite side for causing vision. Whence this experiment may be of use in distinguishing a cataract that admits of the operation from one that is complicated with an amaurosis, or paralysis and insensibility of the nerve: For if, in rubbing the eye in the manner just now mentioned, no coloured circle appears, it is a sign that the eye is paralytic, and that nothing is to be expected from the depression of the cataract: Whereas, on the contrary, the appearance of such a circle plainly shews that the nerve is not affected, and consequently that the success of the operation is not to be wholly despaired of, even though the pupil should be without motion: For such an immobility of the pupil does not always proceed from an insensibility of the nerve, but is also sometimes occasioned either by the pressure of the cataract against the uvea, or by their adhesion; neither of which are esteemed sufficient to render the operation always fruitless.

It is likewise from this principle, that an object seen through a prism appears so far removed from its true place, and that an object seen through a polyedrum or multiplying glass, appears

pears in so many places at once, and therefore appears as often multiplied.

The same principle is also confirmed from the erect and natural appearance of objects, though their image on the retina be inverted: Thus (in fig. 1) H, the lower part of the object GH is projected on *a*, the upper part of the eye, and the highest part G is projected on the lowest part of the eye, which makes the image or representation *ae* inverted; yet, because the point G appears without the eye in the line *e*G, and the point H in the line *a*H, the point G must of necessity be seen higher than the point H.

What hath occasioned some seeming difficulty in the business of erect appearances, is the groundless supposition that the eye, or rather the soul, by means thereof, sees an inverted image of the external object painted on the retina, and that it judges of the object from what it observes in this image: But this is a vulgar error, and I appeal to any one's experience, whether he ever sees any such thing, and every one is himself best judge of what he sees; and, as the mind sees not any image on the retina, so it takes no notice of the internal posture of the retina, or the other parts of the eye, but useth them as an instrument only for the exercise of the faculty of seeing; and therefore, when the retina on its lower part, at the point *e*, receives an impulse or stroke from the rays that come from the upper part of the object G, is it not more natural, as well as useful, that the mind, without any regard to the situation of that part of the

 the

the retina, ſhould, agreeable to the principle here laid down, be directed to conſider the ſtroke as coming from the upper part of the object G, rather than from its lower part H, and conſequently to conclude the cauſe of it, or the object itſelf there alſo? And what is ſaid of upper and lower, holds equally in ſiniſter, dexter, and all other parts of the object.

This may be illuſtrated, by conceiving a blind man, who, holding in his hands two ſticks that croſs each other, doth with them touch the extremities of an object, placed in a perpendicular ſituation: It is certain this man will judge that to be the upper part of the object which he touches with the ſtick held in the undermoſt hand, and that to be the lower part of the object which he touches with the ſtick in his uppermoſt hand.

## SCHOLION.

"The judgment we form of objects being placed without the eye in thoſe perpendicular lines, or, which is nearly the ſame thing, the judgments we form of the ſituation and diſtance of viſual objects, depends not on cuſtom and experience, but on an original connate and immutable law, to which our minds have been ſubjected from the time they were firſt united to our bodies."

That the truth of this may appear, it may be requiſite we look a little into the nature of our ſenſations, and carefully obſerve what is meant in common diſcourſe, when one ſays he ſees an object.

Every

Every body knows, that, properly ſpeaking, colours are the only proper objects of ſight. Now colours may be conſidered five ways.

1. They may be conſidered as the properties inherent in the light itſelf. What theſe properties are, was in the year 1666 firſt diſcovered by the incomparable Newton, and afterwards publiſhed in the Philoſophical Tranſactions, where he alſo gave a ſpecimen of the experiments he made for confirming his doctrine. After that, in the year 1704, the ſame great man propoſed the ſame doctrine more fully, in his beautiful Treatiſe of Optics, and confirmed it with great variety of convincing experiments; where he has demonſtrated, that light is not all ſimilar and homogenial, but compounded of heterogenial and diſſimilar rays, each of which are endued with different properties; ſome, at like incidence, being more refrangible, and others leſs refrangible; and thoſe that are moſt refrangible, are alſo moſt reflexible: And, according as they differ in refrangibility, or reflexibility, they are endued with a power of exciting different motions or agitations in our retina; which, being propagated through the nerves to the ſenſorium, give us thoſe different ideas which we call colours. So that colours, conſidered as properties of light, are not modes or qualifications of light derived from the various terminations of light and ſhadow, or from refractions or reflexions of natural bodies, as was the general opinion of former philoſophers, but original, connate, and immutable properties, which in divers rays are divers: Some

 rays,

rays, when alone, being of fuch magnitude, figure, or folidity, as difpofes them, by ftriking the retina, to exhibit a red colour and no other; fome an orange and no other, and fome a yellow and no other; and fo of the reft of fimple colours, green, blue, indigo, and violet. But, when they are mixed and blended promifcuoufly, they exhibit compound colours of different forts, according as the light is compofed of more or fewer of the different-coloured rays, or as they are mixed in various proportions.

2. Colours may be confidered as qualities refiding in the body that is faid to be of fuch or fuch a colour; and, in this fenfe, colours are nothing but the various difpofitions of the furfaces of objects, whereby they are qualified to reflect only the rays of one fort of colour, or at leaft in greater plenty than the other colours. This was experimented in a dark room, by illuminating objects fucceffively with uncompounded light of divers colours; for by that means any body may be made to appear of any colour. They have there no appropriate colour, but ever appear of the colour of the light caft upon them; but yet with this difference, that they are moft brifk and vivid in the light of their own day-light colour. Minium appeareth there of any colour indifferently, with which it is illuftrated, but yet moft luminous in red; and fo bife appeareth indifferently of any colour with which it is illuftrated, but yet moft luminous in blue; and therefore minium reflecteth rays of any colour, but moft copioufly thofe endued with red;

red; and consequently, when illustrated with day-light, that is, with all sorts of rays promiscuously blended, those qualified with red shall abound most in the reflected light, and, by their prevalence, cause it to appear of that colour. And, for the same reason, bise reflecting blue most copiously, shall appear blue, by the excess of these rays in its reflected light, and the like of other bodies. And that this is the entire and adequate cause of their colours, is manifest; because they have no power to change or alter the colours of any sort of rays incident apart, but put on all colours indifferently with which they are enlightned. But we must remit those who desire full satisfaction in this and the foregoing point, to that admirable treatise of optics written by Newton; for it is impossible to separate the parts of this work from one another, without disadvantage to them, or to sum them up in less room, without losing some things very useful and beautiful; that great person, having before shewn how far numbers and geometry would go in natural philosophy, has, in his optics, manifested to the world to what surprising height even vulgar experiments, duly managed and carefully examined in such hands, may advance it.

3. Colours may be conceived as the passion of our organ of sight, that is, the agitation of the fibres of the retina by the impulse or stroke received from the rays of light; which agitation is communicated to the sensorium, or that part of our brain in which our mind does

does principally reside, else it could perceive nothing.

4. Colours may be considered as the passion, sensation, or perception of the mind itself, or that which all of us perceive, when we look at any object. It is this only that, properly speaking, deserves the name of *colour*; for colours in objects are nothing but their disposition to reflect this or that sort of rays more copiously than the rest, and in the rays of light they are their disposition to excite this or that motion in the organs of vision, and in them they are only different undulations in the animal spirits, which are propagated through the nervous fibres to the sensorium. In all this there is no perception, no sensation, no colour; for, to speak truly, colours are sensations produced in our mind, and do not belong either to light, objects, our organs, or to any other body, but are the modifications of the mind itself, which always stand connected to what passes in the organs and sensorium.

5. The *fifth* manner in which colours may be considered, is the judgment which our mind naturally forms, when it concludes, that that which it feels or perceives is in the body itself said to be coloured, and not in the mind. How body acts upon mind, or mind upon body, I know not; but this I am very certain of, that nothing can act, or be acted upon where it is not; and therefore our mind can never perceive any thing but its own proper modifications, and the various states and conditions of the sensorium to which it is present: For, when

when I look at the ſun or moon, it is impoſſible that theſe bodies, ſo far diſtant from my mind, can, with any propriety of ſpeech, be ſaid to act upon it. To imagine otherwiſe, is to imagine things can act where they are not present; which is as abſurd, as to ſuppoſe that they can be where they are not. *Nam virtus ſine ſubſtantia ſubſiſtere non poteſt. Newton princip. mathemat. ſchol. general. ſub fin.* Theſe bodies do indeed emit light which, falling upon the retina, does excite certain agitations in the ſenſorium, and it is theſe agitations alone which can any way act upon the mind, and therein excite thoſe modifications which we call colours; ſo that it is not the external ſun or moon which is in the heavens which our mind perceives, but only their image or repreſentation impreſſed upon the ſenſorium. How the ſoul of a ſeeing man ſees theſe images, or how it receives thoſe ideas from ſuch agitations in the ſenſorium, I know not; but I am ſure it can never perceive the external bodies themſelves to which it is not preſent: So that, properly ſpeaking, the colours which our mind perceives, when we look at any object, are only the modifications of the mind itſelf, ariſing from the motions excited in the ſenſorium; and, when we imagine we ſee theſe colours in the external object itſelf, this certainly is not a perception, but a judgment or concluſion, whereby we attribute that which our mind feels to external objects.

What we have ſaid with reſpect to colours, may, without difficulty, be alſo applied to our other

other fenfations: For we not only afcribe the colours we perceive to the objects we look at, but alfo we judge that our other fenfations are in the other objects of fenfe; thus, when I tafte fugar, I conclude it is fweet; and when I fmell camphire, or fuch like odoriferous bodies, I am naturally inclined to look on that fmell, which I perceive, as a quality inherent in the object; and yet it is certain that thefe different fenfations are only the modifications of the mind itfelf, and do not belong to the objects to which they are generally attributed.

It is indeed true, that our mind does not always attribute its own proper fenfations to the external objects that produce them; for fometimes it afcribes them to the organs, at other times both to our organs and the object.

That we may explain how this happens, we muft confider that the fenfations of our mind are of three different forts; fome are very ftrong and lively, others weak and faint, and a third fort are of a middle nature betwixt both.

Our ftrong and lively fenfations are thofe that touch our mind very fenfibly, and, as it were, furprife it and roufe it up with force and vigour; and of this fort are all thofe fenfations which are very agreeable or painful, fuch as that which arifes when one is tickled with a feather or burnt with the fire, and in general when the organs fuffer any thing that is capable of hurting or deftroying the body; in all thefe cafes our fenfations are fo brifk and lively, that the mind can fcarce hinder itfelf from looking on them as in fome fort belonging to itfelf;

itself; and therefore it does not judge that they are in the objects, but believes them to be in the members of its body, which, because of the strict union betwixt mind and body, it considers as a part of itself. Thus, when my hand is pricked with a pin, burnt, or otherwise hurt, I am naturally directed to attribute that painful sensation to that part of my hand on which the impression is made, and not to the fire, pin, or other object producing it, and yet it is certain, that pain is a sensation or modification of the mind itself, and belongs as little to our organs as colours do to objects.

The *second* sort of sensations are the weak and languishing, in which the mind is but very slightly touched, and which are neither very agreeable nor disagreeable, as light, when not too strong; all manner of colours, tastes, and smells, moderate sounds, *&c.* These sensations do so slightly affect our mind, that it never thinks that they belong to it, nor that they are in the body to which it is united, but only in the external objects that produce them. It is for this reason we rob the mind of its own proper sensations of light and colour, therewith to deck and imbellish the object; and yet there is nothing more evident, than that all of them are the modifications of the mind itself, and noways inherent in the objects, in which our mind places them.

The *last* sort of our sensations are neither strong nor faint, but of a middle nature betwixt both, such as heat and cold when moderate, great light, violent sounds, *&c.* And here it may be

be obferved, that a weak and languifhing fenfation may become both a middle or ftrong one; as for inftance, the fenfation of light is weak, when the light of a candle or flambeau is faint, or when it is at any confiderable diftance; but, this fenfation may become a middle one, if the flambeau be brought near enough the eye for to dazzle; and likewife it may become very ftrong and vivid, provided that it be brought fo near as to burn. Thus the fenfation of light may be weak, ftrong, or betwixt both, according to its different degrees. Now thefe middle fenfations do neither touch the mind very fenfibly, nor very flightly; hence it is that it is very much embarraffed and ftraitned where to place its fenfation; for, upon the one hand, it is inclined to follow the natural judgment of the fenfes, in removing from itfelf, as much as poffible, thefe forts of fenfations, to attribute them to the external objects; but, upon the other hand, it cannot altogether hinder itfelf from looking on them as in fome fort belonging to itfelf, efpecially if they approach to thofe which we have called ftrong and brifk: And this is the reafon that the mind judges that cold, heat, and the other middle fenfations are not only in the ice, fire, and other objects producing them, but alfo in that part of the body itfelf, upon which the impreffions are made.

Thus you fee how the mind never confiders its fenfations as belonging to itfelf, but always attributes them either to the object, our organs, or both, according as they are of a fluggifh, brifk, or middle nature; and though it

it is certain that they are the modifications of the mind itself, yet since our senses are not given us to inform us what things are in themselves, but only what they are relatively to our bodies, it was very necessary that they should incline us to judge of sensible qualities in the manner they do: As, for instance, it is much more profitable for us to feel pain and heat, as in our body, than that we should judge them only in the objects that caused them; for, since they are capable of hurting our members, it was necessary that we should be advertised when they are thereby attacked, in order to secure ourselves therefrom: But it is not so with respect to colours; for they do not ordinarily hurt the retina on which they fall: And it is altogether useless for us to know that they are painted there. Colours are not necessary, unless to know objects more distinctly, and at a distance; and it is for this reason that our sight ever inclines us to attribute them to objects. From all which it is manifest, that the judgements which our senses induce us to make concerning the sensible qualities, are exceeding just, if considered with respect to the preservation of our bodies, for which they were only given us; though, at the same time, it is most certain, that they are altogether extravagant, and vastly removed from truth.

From what has been said concerning the nature of our sensations, it is evident, that the mind never considers any of them as belonging to itself, but as belonging to something external. Now, since there is no essential or necessary connection betwixt these perceptions and

and the judgements we form concerning them, it follows that these judgments must either depend upon custom and experience, or on an original, connate, and immutable law.

That all of them should depend on custom and experience, is a contradiction in terms, it being impossible for us to have any experience, till some how or other we have formed a judgement; which judgement must therefore depend on an original, connate, and immutable law, that cannot but obtain at least in some of our sensations. To say otherwise, is to say something very absurd: It is to say we judge by experience that has never been experienced. Hence it is plain, that, when one says the mind by custom and experience comes to conclude what it sees to be without the eye in such perpendicular lines; this experience cannot be meant of sight, but of some of our other senses, such as that of feeling or touch; which therefore, by virtue of a connate and immutable law, must naturally, and of itself, without any assistance from the other senses, form a judgement concerning its own perceptions, and conclude, that they are not in the mind but in something external.

But, if by the touch alone we can judge thus of the situation and distance of external things, I see not why the same power should be denied to the sight. It cannot be said that it is more difficult for the mind to trace back the perceptions it has by sight, from the sensorium to the retina, and from thence along those perpendicular lines, to the object itself, than it is to trace back the perception it has by touch from

from the ſenſorium along the nerves to the external object occaſioning them. The ſubjecting our minds to a law in ſeeing, is as eaſy as ſubjecting them to a law in feeling; and that, in feeling, our mind muſt be ſubjected to a law, directing it to conſider its own ſenſations, as belonging to external objects, has been juſt now ſhewn; and this ſhews the tenor and courſe of nature, and makes it highly probable that the ſame thing obtains in our other ſenſes; for nature is very conſonant and conformable to herſelf, and very ſimple and uniform. She never performs ſimilar and like effects in divers manners, and by diſſimilar and unlike means; this were to deſtroy that ſimplicity and uniformity, which is the beauty of all her works, and which ſhe is obſerved to delight ſo much in.

Were not the mind, in ſeeing, ſubjected to a law, whereby it traces back its own ſenſations from the ſenſorium to the retina, and from thence along the perpendicular lines above-named to the object itſelf, and thence concludes what it perceives to be in the external object, and not in the mind, it is plain, that a man born blind, being made to ſee, would at firſt have no idea of diſtance or ſituation by ſight. The ſun and ſtars, the remoteſt objects, as well as the nearer, would ſeem to be in his eye, or rather in his mind; and, if ſo, whence is it he comes afterwards to judge what he ſees to be in the external object? This cannot proceed from experience alone; for, though by the touch we have frequently experienced the exiſtence, diſtance, and ſitua-

tion of things external, and found these ideas to have been preceeded by certain corresponding visible ideas, I see not how, upon perceiving any visible idea present with our mind, we should judge it to be without in the external object, without subjecting our mind to an arbitrary irresistible law directing it so to do. This were to establish an essential and necessary connection betwixt these judgements, and the experiences we have by touch laid up in our memories; whereas it is plain no such thing can be, all the connection that is being only customary and experimental: And, seeing nature at any rate must be at the charge of a law, is it not more reasonable to suppose, that, by the sight alone, without any assistance from the other senses, the mind, in consequence of such a connate and immutable law, as has been allowed it in the judgements it forms by touch, should be enabled to trace back its own perceptions in the so often named perpendicular lines, to the object itself, and thence to form a judgement of its distance and situation? I say, is not this more reasonable, than to suppose that we stand in need of the experiences of touch? Could these experiences be of any use without a new law, there might be some pretence for such a supposition; but, this being impossible, it follows that the judgements we form of the situation and distance of visual objects, depend not on custom and experience, but on an original, connate, and immutable law, to which our minds have been subjected from the time they were first united to our bodies. To say otherwise, is to say that our

our ſight is leſs perfect than our touch; not from any neceſſity in the thing itſelf, but from an error or miſtake in nature. It is to make nature do ſomething in vain, and to be luxuriant in ſuperfluous cauſes, which is to break down the catholic and fundamental rules of philoſophizing, eſtabliſhed by Newton in his *principia philoſophiæ*. It is to maintain, that one accuſtomed from the birth to ſee objects through a priſm or polyedron, and not otherwiſe, would ſee objects as other men do, without any ſuch interpoſi-tion, which to me ſeems very abſurd and ridiculous.

From what has been ſaid, I think the truth of our ſcholium is ſufficiently evident; but, becauſe Dean Berkeley, in his *Theory of Viſion*, has a good deal of ſubtile reaſoning in favour of cuſtom and experience, that has not yet been taken notice of, I ſhall ſtop a little to conſider it.

He obſerves that, in ſeeing, the mind, ſtrictly ſpeaking, perceives nothing but what is preſent with it; and thence concludes, that the ideas of ſpace, outneſs, diſtance, and of the ſituation and magnitude of things placed at a diſtance, cannot by the ſight alone be introduced into our minds; but that having of a long time experienced certain ideas perceivable by touch, to have been connected with certain ideas of ſight, we do, upon perceiving theſe ideas of ſight, forthwith conclude what tangible ideas are, by the wonted ordinary courſe of nature, like to follow: And thus, by a habitual or cuſtomary connection that has grown up between theſe two ſorts of ideas, the latter is always ſuggeſted by the former; juſt as things are

are ſuggeſted by words, and ſhame by bluſhing: So that, according to him, when a man is ſaid to ſee the magnitude and ſituation of this or that diſtant object, nothing is meant, but that the ideas of ſight which are preſent with the mind, ſuggeſt to his underſtanding, that, after having paſſed a certain diſtance in ſuch a direction, to be determined by the motion of his body, which is perceivable by touch, he ſhall come to perceive certain tangible ideas, which have been uſually connected with them; but, becauſe there is no eſſential or neceſſary connection between the ideas of ſight and touch, the ideas, ſuggeſted by ſight, of the diſtance, ſituation, and magnitude, of external things, muſt depend entirely on cuſtom and experience; for, that one idea may ſuggeſt another to the mind, it is ſufficient that they have been obſerved to go together, without any demonſtration of the neceſſity of their co-exiſtence, or without ſo much as knowing what it is that makes them ſo to co-exiſt: Thus words ſignify things, for no other reaſon, than barely becauſe they have been obſerved to accompany them.

In anſwer to this, I muſt aſk Mr Berkeley, how it appears that the ideas we have by ſight of the diſtance, ſituation, and magnitude, of external things, are nothing but the tactile ideas ſuggeſted to our minds? When my eyes are ſhut, I can at pleaſure recal to mind the ideas of touch, which former ſenſations had lodged in my memory, the bare naming the thing doth preſently ſuggeſt them as well as the ſeeing it: But there is no body who doth not

not perceive the difference in himſelf between actually looking upon an object, and contemplating the idea he has of it in his memory; and therefore he hath certain knowledge they are not both memory or fancy. But this is not all: For,

Though it muſt be acknowledged, that, in ſeeing the objects, the mind, ſtrictly ſpeaking, perceives nothing but what is preſent with it; yet it does not from thence follow, that the ideas of ſpace, outneſs, diſtance, and of the ſituation and magnitude of things placed at a diſtance, cannot, by the ſight alone, be introduced into our minds. Were this true, I ſee not how we could ever come by thoſe ideas; for the tangible ideas are as much preſent with the mind as the viſible ideas, and on that account muſt be equally incapable of introducing the idea of any thing external. When with my hand I touch an object, the idea perceived is preſent with my mind; and, in moving my hand along the object, or in moving my body from one place to another, the ideas or perceptions that ſucceed one another are all of them as much preſent with my mind as any viſible idea can be. How then can it be ſaid, that external diſtance and ſituation are only determined by the motion of the body, perceivable by touch? This is to deſtroy the univerſally received notions we have of things, and to confound external ſpace, diſtance, and ſituation, with a ſeries of ideas ſucceeding one another in the mind. It is to take away all difference between ſpace and time, and to make both conſiſt in a conſciouſneſs of a ſucceſſion

cession of different ideas or perceptions in the mind; whereas it is certain, that neither of them depends on our ideas, but must continue the same whether we have any ideas or not. It is to introduce a wild and unbounded scepticism a scepticism that at once banishes this externa world, and space itself, out of the field of existence, and in place thereof substitutes a visionary world of ideas and phantoms, existing no where but in his own mind. It is to suppose that it is GOD who presents the appearances of things external to us, and that he does it in such a manner as to deceive us: For, supposing things external to exist, I see not what greater assurances we could have of their existence than we now have. In fine, this is not to solve the problem, whether it be from custom and experience, or by virtue of an original connate law, that by sight we come to judge of the situation of external things, but, by exterminating all things external, to make the problem itself absurd and ridiculous.

If what has been demonstrated in the preceeding lemma be duly attended to, it will not be difficult to explain how objects seen with both eyes appear single, from their being seen by each of them in the same place.

For illustrating this, let it be supposed, that both eyes are directed to the point F, (See Fig. 1.), by the preceeding lemma, this point must be seen by the left eye in its axis CF, and by the right eye in its axis CF: And, since we have also a faculty of judging of the distance of objects, it follows that the point F must be seen

ſeen by both eyes in that preciſe place where the lines *c*F, CF interſect each other; and, being ſeen in the ſame place by both eyes, it muſt neceſſarily appear ſingle, it being impoſſible for us to conceive two objects exiſting in the ſame place at the ſame time.

In like manner, all the other viſual points of the object GH muſt alſo appear ſingle; for, ſuppoſing the eyes to continue their former direction, the rays, which come from any other point, as H, will be united on the retina of the left eye on the outſide of the optic axis at *a*, and in the other eye they will be united on the inſide of its axis at A: Hence the point H will be ſeen by both eyes in the lines *a*H, AH, which are perpendicular to the retina, at the points *a* and A, where the image is painted; but the lines *a*H and AH meet, and interſect each other at H: And therefore, ſince our mind or viſive faculty has a power of judging rightly of the diſtance of objects, it follows, that the point H muſt be ſeen by both eyes in the preciſe point H, where theſe lines interſect each other; and conſequently muſt appear ſingle, becauſe we can have no idea of the penetration of matter; or, which is the ſame thing, it is impoſſible for us to conceive two viſible objects placed in the ſame place at the ſame time.

What hath been juſt now ſaid of the ſingle appearance of objects ſeen with both eyes, holds only with reſpect to objects placed in the plan of the horopter; for all objects placed out of this plan, muſt, in conſequence of the principle laid down in the foregoing lemma, be ſeen in

in two different places, with relation to that point of the horopter on which our eyes are fixed; and, being ſeen in two different places, muſt therefore appear double.

Thus, if, while the optic axes, (See Fig. 4.) AC, BC, are directed to a mark C, for viewing it accurately, we attend to an object *x*, placed any where within the angle ACB formed of the optic axes, the object *x* will appear in two places; for, being ſeen by the right eye in the direction of the viſual line B*x*, it muſt appear on the left ſide of C, and its diſtance from C will be meaſured by the angle CB*x*; and, being ſeen by the left eye in the direction of the viſual line A*x*, it muſt appear on the right ſide of C, and its diſtance from C will be meaſured by the angle CA*x*; and conſequently it muſt appear double, and the diſtance between the places of its appearance will be meaſured by the ſum of the angles CB*x*, CA*x*. For the like reaſon, ſo ſoon as the eyes change their direction from C to *x*, the object *x* ſhall appear ſingle, but all other objects placed within the angle D*x*E made by the optic axes produced will appear double. Thus, the object C, being ſeen in the viſual lines AC, BC, which are on different ſides of the object *x*, it muſt neceſſarily appear double, and the diſtance between the places of its appearance is meaſured by the ſum of the angles CAE, CBD.

The ſame way of reaſoning, applied to objects in all manner of ſituations, will ſhew, that all of them muſt appear double, when placed out of the plan of the horopter; all which is

exactly

exactly agreeable to experience: And this also is the reason why a double appearance will be seen when the end of a long ruler is placed between the eye-brows, and extended directly forward with its flat sides respecting right and left; for, by directing the eyes to a remote object, the right side of the ruler seen by the right eye, will appear on the left hand, and the left side on the right hand. But we are not from this to imagine, that the single appearance of objects placed in the plan of the horopter, arises from the uniform motion of our eyes; for, while the left eye is directed to F (See Fig. 1.) let the other be directed to G or H, it is plain, from the preceeding lemma, that the points G, F, and H, will continue to be seen in the same perpendicular lines EG, CF, AH, they formerly appeared in, when both eyes were directed to F; and, since at the same time we have a power of judging rightly of their distance, it follows, that here also they must appear single, from their being seen in the same place by both eyes.

How this agrees with the double appearance of objects, when either of the eyes is, by the pressure of the finger, or by a spasm or paralysis in any of its muscles, restrained from following the motions of the other, will be shewn hereafter. In the mean time, from what has been already said, we may see the reason why those who, from use and custom, have acquired a power and habit of squinting, see objects single as other men.

But, for the better explaining the nature of squinting, and, from the above established principles,

ciples, to ſet the diagnoſtics and prognoſtics of this diſeaſe in a juſter light than hitherto has been done, it will be neceſſary to take a view of the ſeveral cauſes from which it may proceed, and to determine ſome of the chief optical effects that naturally ariſe from them, whereby the preceeding doctrine will be further illuſtrated and confirmed. And,

*Firſt*, This diſeaſe may proceed from cuſtom and habit, while, in the eye itſelf, or in its muſcles, nothing is preternatural or defective: Thus children, by imitating thoſe that ſquint, and infants, by having many agreeable objects preſented to them at once, which invite them to turn one eye to one, and the other eye to another, do frequently contract a habit of moving their eyes differently, which afterwards they cannot ſo eaſily correct. Infants likewiſe get a cuſtom of ſquinting, by being placed obliquely towards a candle, window, or any other agreeable object capable of attracting their ſight; for though, to ſee the object, they may at firſt turn both eyes towards it, yet, becauſe ſuch an oblique ſituation is painful and laborious, eſpecially to the moſt diſtant eye, they ſoon relax one of the eyes, and content themſelves with examining it with the eye that is next it; whence ariſes a diverſity of ſituation, and a habit of moving the eyes differently.

In this caſe, which may admit of a cure if not too much confirmed, it is evident, that objects will be ſeen in the ſame place by both eyes, and therefore muſt appear ſingle as to other men; but becauſe, in the eye that ſquints, the

image

image of the object to which the other eye is directed, falls not on the most sensible and delicate part of the retina, which is naturally in the axis of the eye, it is easy to see that it must be but faintly perceived by this eye. Hence it is that, while they are attentive in viewing any object, if the hand be brought before the other eye, this object will be but obscurely seen, till the eye change its situation, and have its axis directed to it; which change of situation is indeed very easy for them, because it depends on the muscles of the eyes, whose functions are entire, but, by reason of the habit they have contracted of moving their eyes differently, the other eye is at the same time frequently turned aside, so that only one at a time is directed to the object.

That all this may be the better perceived, for an object, cause them to look at the image of the upper part of your nose in a plain mirror, while you stand directly behind them, to observe the direction of their eyes.

*Secondly*, The strabismus may proceed from a fault in the first conformation, by which the most delicate and sensible part of the retina is removed from its natural situation, which is directly opposite to the pupil, and is placed a little to a side of the axis of the eye, which obliges them to turn the eye away from the object they would view, that its picture may fall on this most sensible part of the organ. Thus, if *a* (See fig. 1.) be supposed to be the most sensible part of the retina, in order to see the object H, the eye must be turned aside to F.

When this is the case, the disease is alto-

 gether

gether incurable, and the phænomena that ariſe therefrom differ in nothing from the phænomena of the former caſe; excepting only that here, 1*mo*, The object to which the eye is not directed will be beſt ſeen, which is the reverſe of what happens when this diſeaſe ariſes barely from habit and cuſtom. 2*do*, No object will appear altogether clear and diſtinct; for all objects to which the eye is directed, by having their image painted on the retina at the axis of the eye, where it is not very ſenſible, will be but obſcurely ſeen; and objects that are placed ſo far to a ſide of the optic axis, as is neceſſary for making their image fall on the moſt ſenſible and delicate part of the retina, muſt appear a little confuſed, becauſe the ſeveral pencils of rays that come therefrom fall too obliquely on the cryſtalline, to be accurately collected in ſo many diſtinct points of the retina; tho' it muſt be acknowledged, that this confuſion is for the moſt part ſo ſmall as to eſcape unobſerved.

*Thirdly*, This diſeaſe may proceed from an oblique poſition of the cryſtalline, as in fig. 5. where the rays that come directly to the eye from an object at A, and that ought to converge to the point of the retina D, which is in the axis of the eye, DE, are, by reaſon of the obliquity of the cryſtalline, made to converge to another point, as C, on that ſide of the viſual axis DEA, where the cryſtalline is moſt elevated; and therefore the object is but obſcurely ſeen, becauſe its image falls not on the retina at the axis of the eye, where it is moſt ſenſible. But the rays that fall obliquely on the

the eye, as thoſe that come from an object at B, will, after refraction, converge to this moſt ſenſible part of the retina D, and, by converging there, muſt impreſs the mind with a clearer idea of the object from whence they came. It is for this reaſon that the eye never moves uniformly with the other, but turns away from the object it would view, being attentive to the object to which it is not directed. When this is the caſe, it is in vain to expect any good from medicine.

The ſymptoms that naturally ariſe from it are, 1*mo*, The object A, to which the eye is directed, will be but faintly ſeen, becauſe its image falls on the retina at C, where it is not very ſenſible. 2*do*, The object B, to which the eye is not directed, by having its image painted on the retina at the axis of the eye DE, will be clearly perceived. But, 3*tio*, This ſame object B muſt appear ſomewhat indiſtinct, becauſe the pencils of rays that flow from it are not accurately collected in ſo many diſtinct points in the retina, by reaſon of their oblique incidence on the cryſtalline. And, 4*to*, It muſt be ſeen, not in its proper place B, but thence tranſlated to ſome other place, as A, ſituated in the axis of viſion DEA. (See the preceeding lemma and ſcholium.) And, 5*to*, Being thus tranſlated from its true place, where it is ſeen by the other eye that does not ſquint, it muſt neceſſarily appear double, and the diſtance between the places of its appearance will be ſtill greater, if the cryſtalline of the other eye incline to the contrary ſide.

Had Monf. de la Hire attended to this tranflation and double appearance of the object, I fee not how he could have imagined, that, when the cryftalline is inclined differently in both eyes, the moon, for example, will appear like two ovals that decuffate one another, as in Fig. 6. (See his differtation *fur les differens accidens de la vüe.*). For, though the moon or any round body may appear a little oval from the unequal refraction of the rays in the oblique cryftallines, and though thefe ovals may have different inclinations arifing from the different inclinations of the cryftallines; yet they can never decuffate one another, but, being tranflated to two different places, muft neceffarily appear double, and at a diftance from each other.

*Fourthly*, This difeafe may arife from an oblique pofition of the cornea, which, as Maitre-Jan obferves, in this cafe is commonly more arched and prominent than what it is naturally. When the eye has this conformation, no object to which it is directed can be clearly feen, becaufe its image falls not on the retina, at the axis of the eye, and therefore the eye turns afide from the object it would view, that its image may fall on the moft fenfible part of the retina.

To determine the fituation of the eye, with refpect to the object it would view, let AGK be the axis of vifion, (See Fig. 7.), and let the arch *bGd*, whofe center is *c*, reprefent the oblique prominent cornea, and let *aAx* be a cone of rays, having its bafis in the pupil, and its apex in the moft fenfible part of the retina;

retina; it is evident that this cone muſt come from a point without the eye, which of all others appears moſt clear and diſtinct. To find therefore the ſituation of this point, nothing is required but to trace back the middle ray AG, in the line it deſcribed before its incidence on the cornea at G. For this purpoſe erect upon the ſurface of the cornea, from the point of incidence G, the perpendicular GP, and having produced it downwards to Q, from A let fall upon it the perpendicular AD, and produce it to H; ſo that DH may be to AD as the ſine of incidence to the ſine of refraction, that is, as 4 to 3; and about the centre G, with the radius GA, deſcribing a circle AHP, draw, parallel to the perpendicular GPQ, the line HE cutting the circumference in E, and join EG; this line EG ſhall be the line of the incident ray, in which an object muſt be placed to have its image painted on the retina at the axis of the eye A, where it is moſt ſenſible. For if EF be let fall perpendicularly on the line FQ, this line EF ſhall be the ſine of incidence of the ray EG, the angle of incidence being EGP, and this ſine EF is equal to DH, and conſequently in proportion to the ſine of refraction AD, as 4 to 3.

When the ſtrabiſmus proceeds from this cauſe, the prognoſtic and phaenomena that attend it will be much the ſame as in the caſe immediately preceeding, from which nevertheleſs it may be diſtinguiſhed by the obliquity of the cornea, which is manifeſt to the ſenſes; and, if the cornea be alſo more arched and prominent than what it is naturally, which is

commonly the caſe, the eye will alſo be ſhort-ſighted.

*Fifthly*, This want of uniformity in the motions of our eyes may ariſe from a defect, or any great weakneſs or imperfection in the ſight of both, or either of the eyes.

It will afterwards be made appear, that the motions of our eyes are voluntary, and depending upon our mind, which being a wiſe agent, does will them to move uniformly; not from any abſolute neceſſity in the thing itſelf, or for want of power to move them differently, but becauſe of the utility and advantage that ariſes from ſuch motions. Hence children, for ſome time after birth, may be obſerved to look differently with both eyes, till, from the advantage they find in moving them the ſame way, they come at length to move them always uniformly; which uniform motion does at laſt become ſo neceſſary, as to render any other motion impoſſible, it being certain that cuſtom and habit does render many actions neceſſary which are not eſſentially ſo.

This being the caſe with reſpect to the uniform motion of our eyes, it follows, that, when the ſight of both or either of the eyes is ſo defective, weak, or imperfect, as to diſappoint us of the advantages that naturally ariſe from their uniform motion, the mind will not accuſtom itſelf to this ſort of motion, but will preſerve its natural and connate liberty of moving them differently. An example of this may be had in thoſe who, from a cataract or *gutta ſerena*, have been blind from the birth in both or either of their eyes; and that the ſame

ſame thing may alſo happen when the diſeaſe is of a later date, ſeems evident from what Plempius obſerves (*Ophthalmograph. lib.* iv. *problem* 13.) of a girl who became blind from a *gutta ſerena* in the fifth year of her age: This girl, finding no further advantage, after being blind, in moving her eyes the ſame way, came at laſt to forget the habit ſhe had formerly acquired in the four years ſhe ſaw, and moved her eyes differently, ſometimes their axes being parallel, ſometimes converging, and at other times diverging.

The prognoſtic in this caſe is the ſame with that of the diſeaſe from which it proceeds, and the phaenomena are ſo obvious from what has been already ſaid, that to ſay any more, would be but to make repetitions.

*Sixthly*, Another cauſe from which the ſtrabiſmus may proceed, lies in the muſcles that move the eye. When any of thoſe muſcles are too ſhort or too long, too tenſe or too lax, or are ſeized with a ſpaſm or paralyſis, their equilibrium will be deſtroyed, and the eye will be turned towards or from that ſide where the muſcles are faulty.

In this caſe, the diſeaſe frequently yields to medicine, and therefore admits of a favourable prognoſtic; excepting only when, by a fault in the firſt conformation, any of the muſcles are longer or ſhorter than their antagoniſt; in which caſe, if ever it ſhould happen, no medicine can be of any value.

As to what concerns the optical phaenomena, they are the ſame here as in caſe firſt; only, when the diſeaſe commences not till, by cuſtom

ftom and habit, the uniform motion of the eyes has been rendered neceffary, all objects do for fome time appear double, of which we have given fome examples above.

To explain how this fhould come to pafs, and at the fame time to reconcfle it with what has been formerly faid, from *p.* 158. to *p.* 195. concerning the fingle appearance of objects feen with both eyes, where I have demonftrated that this fingle appearance does not depend upon the uniform motions of our eyes, is a matter of very great difficulty, and therefore deferves well to be very carefully examined.

It hath been fhewn that objects are feen without the eye, in lines drawn perpendicularly to the retina, from that point of it where their image is painted; it hath likewife been demonftrated, that thefe lines continue invariably the fame without any change of fituation, though the eye be turned away from the object to which the other eye is directed: Whence it feems to follow, that the object in all fituations of the eye fhould continue to be feen in the fame place by both eyes; and being feen in the fame place, fhould always appear fingle: How comes it therefore that, in the cafe before us, the object fhould appear double? What is it can put this cheat upon the underftanding? Whether the eyes look the fame way or differently, the object appears to each of them in the fame lines; and fince, at the fame time, we alfo perceive its diftance, it ought to appear fingle, becaufe feen in the concourfe of thefe lines, and yet it is efteemed double: Herein confifts the difficulty, which does vanifh and

admit

admit of a most easy solution, if we but consider that the mind mistakes the situation of the eye, and supposes that it is directed to the same object with the other; for, having been accustomed to move both eyes uniformly, from which a kind of necessity has arisen that makes it impossible for us to move them differently, when we would view any object, the mind wills both eyes to be directed towards it, and, not being sensible that either of them has disobeyed its commands, it concludes that they are both turned the same way, because it has been accustomed to find them so upon the like occasions: And being thus mistaken as to the situation of the eye, it must also be mistaken as to the situation of the object seen by it, which therefore must appear double, by reason of the different situation it appears in to each eye, it being impossible for us to conceive that any one thing can be in two different places at the same time.

To illustrate this matter, let it be supposed, that one wills both eyes to be directed to F, (See Fig. 1.), for viewing it accurately, and that, while the left eye gives ready obedience, let the other, by reason of a recent defect in some of its muscles, be turned to H: It is plain that the point F will be seen in the same perpendicular line CF it would have appeared in, had it been directed to F; but, because this line, by reason of the obliquity of the eye, does not fall on the retina at its axis C, but at some other point on the outside of this axis as E, so that the angle C*o*E may be equal to the angle F*o*H, it will itself appear translated to

EG,

EG, and being thus tranflated, the point F muft be tranflated with it, which therefore will be feen, not in its proper place F, but in fome other place as G, fituated in the perpendicular E$o$G. For, fince the mind knows not but the eye is directed to F, it muft form the fame judgment with refpect to the fituation of objects, as if it were really fo: But it has been already fhewn, that objects are always feen by virtue of a connate immutable law, in lines drawn perpendicularly to the retina, from that point of it where their image falls; and therefore the object F, having its image painted on the retina at E, muft be feen by this eye, which the mind fuppofes directed to F, fomewhere in the perpendicular EG, as at G; while to the other eye it appears in its true place F: And, being thus feen in two different places, G and F, it muft neceffarily appear double.

Something like this happens when an object is felt, by the extremities of two fingers that are made to crofs each other; for the mind, not attending to the pofition of the fingers, judges that the object is double; becaufe it is felt by thofe parts of the fingers that ufe to be at a diftance from each other.

That we have here given the true account of this phænomenon, will be further evident to any one who confiders, that, when the mind does not miftake the fituation of the eye, as in thofe who by cuftom have from their infancy contracted a habit of moving their eyes differently, all objects appear fingle as to other men; and this likewife is the reafon why, in the cafe before us, all things come in time to

to be seen single: For, by repeated experiences, the mind becomes wiser, and, by degrees, learns to form a right judgment concerning the direction of the eye, which judgment, by becoming habitual, must serve to correct the mistake it was formerly led into with respect to the situation of objects. The ingenious Mr Chefelden gives us a very beautiful example of this in his system of anatomy; his words are "A gentleman who, from a blow on the head, "had one eye distorted, found every object appear double; but, by degrees, the most familiar ones became single, and in time all objects became so, without any amendment of the distortion."

The not considering of what has been said, has been a fundamental and perplexing oversight; for proof whereof, we need go no farther than the case before us; for explaining which many learned men have been forced to suppose, that by sight alone we can never form any judgment with respect to the situation of objects, without calling in the experiences of touch, the contrary of which has been set forth at large, and demonstrated in the preceeding scholion.

From all that has been said on this head, laid together and duly considered, we may clearly deduce this inference: The double appearance of objects that happens when either of the eyes is, from a spasm or paralysis of any of their muscles, or from any other cause, restrained from following the motions of the other, does not prove, that, to see objects single, it is absolutely requisite that both eyes be directed

rected to the fame object, and that this is one of the final caufes of their uniform motion.

*Laftly*, This want of uniformity in the motions of our eyes may proceed from a preternatural adhefion or attachment to the eye-lids; of this we have already given an example from Langius. And that the fame thing may alfo be occafioned by a tumour of any kind within the orbit, preffing the eye afide, and reftraining it from following the motions of the other, is fo evident, that I need not bring inftances to prove it; thofe who defire fuch, may confult obfervators. Here alfo the cafe may admit of a favourable prognoftic: And as for what concerns the optical phænomena, they muft be the fame as in the cafe immediately preceeding.

Having finifhed what I intended to fay concerning the final caufes of the uniform motion of our eyes, I come now to inquire into the efficient caufe of this uniformity, or by what neceffity it happens that both eyes are always turned the fame way, fo that none of us are able at pleafure to give them different directions.

Ariftotle of old, and after him Galen, Avicenna, and moft of the antients, do attribute this to the union of the optic nerves, near the *fella offis fphenoides*; but, fince thefe nerves give no branches to the mufcles, but are wholly beftowed upon the retinæ, it follows that they can attribute nothing towards the motion of our eyes, but are only for conveying to the mind, or vifive faculty, the impreffions made upon their fund by the rays of light.

Hence

Hence it is, that, in blindneſs from obſtructions in thoſe nerves, the eyes continue to move as formerly; becauſe their motion does not depend upon the optic nerves, but upon their other nerves and muſcles. But, ſuppoſing that the optic nerves did contribute to the motion of our eyes, yet their conjunction could never occaſion this uniform motion; becauſe, as Diemerbroeck obſerves, (See his Anatomy, *lib.* iii. *cap.* 16.), anatomiſts have found them disjoined in ſome ſubjects, who, while alive, moved their eyes uniformly as other men.

It is therefore, with good reaſon, that our moderns have rejected this hypotheſis as falſe and groundleſs; but neither have they themſelves ſucceeded better, when they tell us, That this happens becauſe the nerves beſtowed upon the muſcles of our eyes, called *oculorum motorii*, are united at their origin in the brain. Every body knows that our fingers are at liberty to execute different motions, and to be extended ſeparately, though not only the nerve, but alſo the muſcle ſubſervient to their extenſion is but one: Whence therefore this liberty ſhould be denied our eyes, whoſe muſcles are diſtinct, I ſee not. But this is not all; for there are many parts of the body, which, though they have nerves of different origins, yet they neceſſarily move together. Thus the eyes cannot be turned up or down, but the eye-lids follow their motion, and keep at the ſame diſtance from the pupil, though at the ſame time the eye-lids can be moved without any motion in our eyes. Did this uniform motion depend upon any union or

conjunction of the *oculorum motorii*, or of any of our other nerves, none would squint but such as had them disjoined; and it would be in vain to use any precaution against childrens taking up such a habit, or to endeavour to correct it.

The true cause of this uniformity in the motions of our eyes to me seems wholly to depend on custom and habit. For it is not to be doubted but these motions are voluntary, and depending upon our mind, which, being a wise agent, wills them to move uniformly, not from any intrinsical necessity in the thing itself, or for want of power to move them differently; but because such motions are most profitable and useful to us. So that our opinion is, that the uniform motion of our eyes is not at first necessary, but that the mind has imposed upon itself that law founded upon the utility and advantage that arises from this sort of motion; which motion does in time become so necessary, that none of us are now able to move one eye towards any object, but the other is likewise turned the same way. And, as for other creatures who move their eyes differently, such as the chameleon, which has this faculty in an eminent manner, so that the one eye is moved whilst the other remains immoveable; the one is turned forwards, at the same time the other looks behind; and the one looks up to the sky, when the other is fixed on the ground: I say, as for other creatures that move their eyes differently, such as the hare, chameleon, &c. it is evident, since the organs subservient to those motions are the same as in man, that it is the utility and advantage they receive

receive from these particular motions, which determines that principle which governs and rules all their motions, to actuate the organs in such a manner as those motions, which they find most profitable and necessary for them, may follow.

Dr Goddart (in the Philosophical Transactions) has observed, that the eyes of the chameleon resemble a lens or convex glass, set in a versatile globular socket, of which our Parisian academists have taken no notice. But, be this as it will, they found that they were moved by true muscles, which, as in other creatures, are inserted under the conjunctiva; so that it seems Panarolus was mistaken, when, as Bartholin informs us, (*Hist. anat. rar. cent.* 2. *hist.* 62.) he says, that their eyes want muscles, and that they are moved by the corrugation of a membrane, which is contracted by means of circular fibres. What might have led him into this mistake, may be guessed at from the observations of the same academists, who tell us, that the eyes, which are very large, jut out full half of their ball, and are covered with one single eye-lid, made like a cap pierced through the middle, with a hole not exceeding one line in breadth. This eye-lid was found fastened to the fore-part of the eye, by means of an orbicular muscle that was spread over the whole *tunica conjunctiva*, to which, as well as to the eye-lid, it was so adherent, that it served to give the same motion to the lid as to the eye, though its particular action was to contract the little round hole of the lid, which closes by enlarging itself cross-

ways, even to the making one single slit, which very exactly unites the upper part with the lower. Seeing then that the eye cannot be moved, without communicating the same motion to the eyelid, which must therefore appear corrugated, it is probable that Panarolus, for want of due scrutiny after the muscles, might have imagined that the motions of the eye proceeded from the corrugation of this membrane, which is indeed contracted by means of the circular fibres of the orbicular muscle. But, supposing Panarolus's observation to have been just, it is all one with respect to the present case; for the dissimilar motion of their eyes, arising from the dissimilar contraction of those circular fibres, can have no foundation, but in the utility and advantage that arises from such motions, which might as well have been executed by muscles. Nor can any good reason be assigned why the mind, which presides over all the animal (if not also the vital and natural) motions, should not be at liberty to contract this or that muscle independently of others, as well as to contract this or that fibre independently of others, especially when we find it frequently does so in other creatures, such as fishes, birds; and amongst quadrupeds, the hare, coney, &c.

And as the hare, chameleon, &c, have a power of moving their eyes differently, so neither is there any room to doubt, but that at first we ourselves are also possessed of the like power; as is evident from the observation, already mentioned, of children, who, for some time after birth, can look different ways with their eyes; which power they retain, till, by discovering

discovering the advantage of directing them the same way, they come to move them always uniformly. This uniform motion, by use and habit, at last becomes so necessary, that the eyes cannot be moved differently; long custom rendering many actions necessary, which were not so essentially, nor from the beginning.

I have already given an example of this in the motions of the upper eye-lid, which always follows the motion of the eye, and keeps at the same distance from the pupil, whether the eye be turned up or down. The same thing may also happen the fingers; for, if one is not accustomed to move any of them but in conjunction with the rest, it will not be in his power to move them separately. Hence it is that most people cannot bend their ring-finger towards the palm of their hand, but the little one shall follow its motion. If any body desires more examples of this kind, let him try to elevate one of his eye-brows, while the other is depressed; let him try to dilate one of his nostrils, or one side of his thorax, while the other is contracted; or, if he can, let him contract the muscles on one side of his belly, while those on the other side continue relaxed. I remember a time when it was very difficult for me to shut any one of my eyes, while the other was open, which now is very easy for me, because I have accustomed myself thereto; and this frequently happens, even in the eyes themselves: For, if we accustom ourselves to direct them different ways, as boys do often in imitating those that squint, we shall

in time be able to ſquint without difficulty, eſpecially if young. This is further confirmed from the diſſimilar motions of the eyes that are ſo frequently obſerved in blind people, and particularly by that hiſtory, already quoted from Plempius, of a girl who loſing her ſight, and having therefore no longer any advantage from the uniform motions of her eyes, came at laſt to move them differently. Hence it is that children, the younger they are, are the more apt to become gogle-ey'd; becauſe, when young they have not ſo much accuſtomed themſelves to look the ſame way with both eyes, as to render that uniform motion neceſſary; and therefore do frequently become gogle-ey'd, by having many pleaſant objects preſented to them at the ſame time, which invites them to turn one eye to one object, and the other eye to another: And thus they contract a habit of moving their eyes differently, which is apt to continue all their lifetime, if not timely corrected. Willis has obſerved this (in his *Anima Brutorum*, cap. 15.) in theſe words: "Quare "infantes, quando ipſorum oculis multæ res "ſimul objiciuntur, ſtrabiſmum facile contra"hunt."

XIII. *Supplement to* Art. XI. *of* Vol. I.; *by* ALEXANDER MONRO, *Profeſſor of Anatomy in the Univerſity of* Edinburgh.

HAVING converſed with ſome younger anatomiſts, whoſe imagination could not ſupply the want of a figure to explain what I wrote

vrote in Art XI. of your firſt volume, con-
cerning the digaſtric muſcles, I beg you would,
n their account, publiſh the incloſed figures,
vith a reflection or two on them, as a ſupple-
nent to that paper.

Tab. II. fig. 1. is a copy of as much of Cow-
er's tab. 23. *Myot.* as relates to the parts in
queſtion, with the addition of two or three
ricked lines; the view here repreſented being
direct one of the left ſide, with the head re-
lined towards the right, and the muſcles in an
nactive ſtate.

A, denotes the baſe of the lower jaw, to
hich the anterior head of the digaſtric muſcle
fixed.

B, The part of the temporal bone from which
ie other head of the digaſtric riſes.

C, The *os hyoides.*

D, The internal jugular vein filled with
vax.

E, The anterior head of the digaſtric mu-
le.

F, The poſterior fleſhy belly of the ſame muſ-
le.

G, *g*, The middle tendon common to both
ieſe heads.

H, A ſtrong aponeuroſis ſent from that ten-
on to the *os hyoides.*

*i*, Part of that tendon raiſed into a convexity
orwards, by the over-diſtended jugular vein.

K, The *ſtylo-hyoid* muſcle, through which
ie tendon of the digaſtric paſſes.

L, The extremity of the *ſtylo-hyoid* muſcle,
iat is fixed to the *os hyoides.*

Fig.

Fig. 2. Reprefents the direct anterior view of the *ftylo-hyoid* and digaftric mufcles, with the *os hyoides*, when the head is drawn much back.

The letters common to this figure, with the former, denoting the fame parts, a comparifon of the two figures will fhew what the effect of the different attitudes is.

Befides what is common to both, *q, q*, here point out the great curve of the round tendons, to be at the fame part where the ftrong aponeurofis H goes off to the *os hyoides*.

P is the thin aponeurofis of the digaftric mufcles, fpread upon the mylo-hyoid mufcles.

You fee then that Mr Cowper paints no ligamentous pulley here for the round tendon of the digaftric mufcle to move in; nor has he any in tab. 31. *Myotom.* where the digaftric is again reprefented; but in both he delineates the aponeurofis fixed to the *os hyoides*, which he mentions in his defcription, num. 68. as a conftant thing, as I always find it. But he is obliged to acknowledge (notwithftanding the fondnefs he expreffes for being the firft who fhewed the mechanifm by which the digaftrics act on the lower jaw) that the tendon paffes through the ftylo-hyoideus, and an annular ligament, or fometimes a membranous inclofure. I ftill affirm, that I never faw any ligament, fuch as ties down the mufcles of the hand, fingers, foot, toes, or any other mufcle, whofe action is determined by a band, or ligament, to any direction different from the ftraight courfe of its flefhy fibres, and that what is called the annular ligament of the tendon of the

the digaſtric muſcle, is no other than the common cellular membrane, which ſtretches with the leaſt force drawing it, and readily riſes into cells upon blowing air into it.

Some have fancied that the ſtylo-hyoideus muſcle ſerves as a pulley to the tendon of the digaſtric; but any who have ever diſſected muſcles, muſt know that their fleſhy fibres are connected to each other by ſuch weak fibres, as can make very little reſiſtance; and, in diſſecting this part of the ſtylo hyoideus muſcle, we ſcarce can hold it with a hook or forceps ſo tenſe as to diſſect it clean, without the riſk of tearing its fibres aſunder, which painly ſhews it to be too weak for the uſe they aſſign it. They ought to have alſo conſidered, that the tendon has no curve where it paſſes through this muſcle, which is a ſure ſign that the muſcle does not ſerve as a pulley to it.

Though there was a pulley, through which this tendon paſſed, it could be of no uſe, ſo long as the tendon is tied to the *os hyoides*; for the aponeuroſis, H, hinders its play in the ſame way as threads tied to a cord laid over a pulley, and to the ſcrew or ropes by which the pulley is ſuſpended, prevent motion of the cord.

It has been ſaid, that, though there is a great curve in the middle of each digaſtric muſcle, when the head is extended backwards, yet it becomes ſtraight when the head is bended forwards; and that, when theſe muſcles are ſtraight, they may bring the lower jaw down.——To this it may be replied, 1. That the digaſtric muſcles never are ſtraight; there is a curve in them

them when the chin touches the breaſt. 2. If they were to pull the jaw down, they muſt draw the *os hyoides* upwards and backwards; whereas this bone deſcends and advances forwards when the mouth is opened. 3. In this ſituation of the head, bended ſo greatly forwards, which is ſuppoſed to be the proper one for the action of the digaſtric muſcles, the mouth is not opened by the deſcent of the lower jaw, but by the upper one only being raiſed.

One may readily judge from the figures, what the action of this digaſtric muſcle muſt be. If the poſterior head, F, alone act, the middle tendon will be drawn backwards till the aponeuroſis ſhuffles from *m* to *o*, which is a very ſmall ſpace in Mr Cowper's figure, and muſt be much leſs in mine, after which this head acts on the *os hyoides*.

The anterior belly, E, acting alone, the extremity of the aponeuroſis, H, is brought from *m* to *n*, which is as much as this head could be expected to contract.

If both heads act while the *os hyoides* is not kept down, the muſcle becomes ſtraighter, approaching the line *r*, till its axis is placed in that line, which I have endeavoured to prove, is the proper office of this muſcle in deglutition, in which it is aſſiſted by the ſtylo-hyoideus, K.

If both heads contract while the muſcles, which pull the *os hyoides* down, are in action, the tendon, G, will be raiſed towards *r*, till the aponeuroſis, H, is made ſtraight and tenſe. Both bellies then continuing to contract in this ſituation,

ſituation, they will conſpire in their efforts to draw the *os hyoides* upwards; beſides which, the anterior head pulls alſo the jaw, A. Before the jaw can be depreſſed, the *os hyoides*, C, muſt be ſo firmly kept down by its muſcles, that it ſhall be more difficultly raiſed than the jaw can be depreſſed, otherwiſe E would move the *os hyoides* upwards, without any effect on the jaw. Since, therefore, the jaw can be depreſſed with no greater force than what is employed to pull the *os hyoides* down, it will neceſſarily follow, that, whatever diminiſhes the force pulling the *os hyoides* down, muſt hinder inſtead of aſſiſting the depreſſion of the jaw. But, in the preſent ſuppoſition of both heads of the digaſtric muſcle, contracting in the ſituation above deſcribed, it is evident that the poſterior head can have no effect on the jaw, but exerts its whole force in pulling the *os hyoides* upwards, by which it counteracts the muſcles, *i. e.* diminiſhes the force that draws the *os hyoides* down, (which the anterior head cannot increaſe), and conſequently the jaw is depreſſed, or the mouth is opened with leſs force, when the digaſtric muſcle contracts, than when it is unactive: From all which, eſpecially if joined to the experiments mentioned in the article to which this is a ſupplement, it appears evident to me, that the depreſſion of the jaw is no more the office of the digaſtric than the extenſion of the fore-arm is the office of the *brachiæus internus*.

ARticle xi. of vol. 1. and this its supplement, having been severely criticized without convincing me that I am in the errors imputed to me, I think myself obliged to offer what arguments I can in support of these articles, to which, on this account, no alterations are made in this edition, tho' my other papers in this collection have corrections and additions made to them.

My first critic is the famous Mr Winslow, one of the greatest masters in anatomy, to whom the world is infinitely obliged for numerous improvements and discoveries, and who appears, from his writings, to be a scrupulously honest man.

The second is Mr Ferrein, who has given several specimens of his accuracy and ingenuity published among the memoirs of the French academy of sciences, and in his other writings.

The third is professor Walther of Leipsic, who is author of numerous anatomical, physiological, and medical tracts, which shew him industrious and learned.

These gentlemen have argued so differently, that I must consider each by himself.

## Answer to Mr WINSLOW.

MR Winslow * observes, 1. That at the end of vol. 1. of your medical Essays, &c.

* Men. de l'Acad. des sciences 1742. p. 178. of 4to Edit. or p. 139. of 12mo.

&c. publiſhed in 1733, notice is taken of his *Expoſition Anatomique*, publiſhed in the year 1732.——2. That Dr Douglas's tranſlation of the expoſition was publiſhed in 1732.——3. That the 3d vol. of eſſays was publiſhed in 1735.

I fancy the meaning of theſe obſervations is, to inſinuate that I was the firſt aggreſſor; and therefore, to ſet this matter in its true light, I muſt remark,

1. That our vol. 1. which is properly for June 1731—May 1732, was publiſhed in 1732 notwithſtanding the bookſellers having put 1733 upon its title page, according to the common practice of people of that buſineſs, when they publiſh books near the end of a year; and that this volume was ſtopped ſeveral weeks at the preſs, after more than two thirds of the ſheets were caſt off.——When my remarks on the articulation, &c. of the lower jaw, were read to the ſociety in 1731, and when they were printed, I knew no more of the expoſition, than that a ſyſtem of anatomy was long and impatiently expected from Mr Winſlow.

2. There muſt be a typographical error in the ſecond obſervation; for 1734 is on the title page of Dr Douglas's tranſlation of the expoſition; nor could a tranſlation of ſuch a book have well been publiſhed ſooner after the original.

3. When vol. 3. of eſſays was publiſhed, I did know Mr Winſlow's opinion concerning the point now in diſpute, but did not ſee reaſon to embrace it.

The obfervations are followed by an accurate account of Mr Winflow's doctrine, and then mine is attacked.

On my argument of the digaftric mufcles being too fhort for drawing down the lower jaw, in opening the mouth *, it is remarked (p. 185. 4to, 249. 12mo.) that the lower jaw being a crooked lever, a fmall motion at its angles muft make a confiderable one at the chin; and that the digaftric mufcles may be regarded as if their infertion was towards its angles; therefore a fmall quantity of contraction of the fibres of thefe mufcles is only required.

All will agree, that the motions of the feveral parts of the lower jaw, and of every other bone, are in proportion to their diftances from their common center of motion; and therefore the angles of the jaw move much lefs than the chin does: But this is no reafon why the digaftric mufcles fhould be regarded as if they were inferted into the angles, when they are inferted into a part of the jaw which has a much larger motion than the angles have, in proportion to which motion they muft contract; and the quantity of this contraction muft be according to the length of their fibres, which I ftill think are too fhort for fuch an office. This is confirmed by the diffection of quadrupeds and birds, whofe digaftric mufcles deprefs the lower jaw; for, in them, thefe mufcles are not inferted into the fore-part of the jaw, but much nearer to its center of motion.

In

* Edinburgh effays, vol. 1. art. xi. p. 109.

In ſtating my ſecond objection, Mr Winſlow has twice omitted the word *proportional*, and from thence is led to criticize me (p. 187. 4to, 251. 12mo) for what I have not ſaid.

I have ſo good an opinion of Mr Winſlow's integrity, that I ſhall give this hint, without entering into any argument, believing that it will be ſufficient to make him wiſh theſe remarks had been omitted.

In anſwer to this 2d objection (p. 187--252) and in ſome other parts of his *memoire*, Mr Winſlow alſo interprets what I called the angle of inſertion of the digaſtric muſcles, as if I meant by it the curvature of the tendons of theſe muſcles.

What has occaſioned this miſtake, I cannot tell; for I have always named them differently: And I thought every one had known what is meant by the angle of inſertion of a muſcle into a bone, to wit, the angle between the muſcle and the bone, and that the nearer this angle is to a right one, the muſcle always acted, *cæteris paribus*, with greater advantage.——In the preſent queſtion it might be eaſily demonſtrated, that the nearer perpendicular a muſcle fixed at one end to the *os hyoides*, and to the chin at the other end, is to a line drawn from the axis of the jaw through the part of the *os hyoides* where the muſcle is fixed, the larger angle it muſt act with in drawing the jaw down; conſequently the digaſtric muſcles act with a larger angle when the head is reclined back, and with a leſs one as the chin is depreſſed; that is, theſe muſcles have a leſs angle of inſertion, as they continue to act; whereas moſt other muſcles obtain a lar-

ger angle of insertion as they act. Thus, when the fore-arm is extended, the biceps and *brachiæus internus* are inserted at a very acute angle; but, as the fore-arm bends, the angle of their insertion increases.

Whether the aponeurosis of the digastric muscles is long and loose enough to shuffle as much as is sufficient for their action, if they were depressors of the jaw, is the next point in which Mr Winslow and I differ, and which other anatomists inspecting the parts must determine.

After this I am blamed (*Mem.* p. 189—254.) for considering the actions of each head of the digastric muscles separately; and, by repeating this critical remark afterwards, Mr Winslow seems to think that these muscles always act simultaneously, or at the same time.

If I had neglected to consider the simultaneous action, I had been to blame; but, as this was done, there could be no fault in examining what each head can do; because I have no doubt of their often acting separately, as I see different parts of other muscles act, which seem to be more made for co-operation than these two parts of the digastric muscles. Thus, for example, when the arm hanging by the side, is to be raised by the deltoid muscle, its middle part only can act at first, because the fore and back parts of this muscle being then placed below the center of motion of the joint of the arm, might, if they acted, pull down the arm with as much force as the middle part could exert to raise it; and, on the contrary, when the arm is to be pressed to the ribs, the lateral parts of the deltoid muscle contract, while the middle part is inactive.

In

In relating my 4th objection (*Mem. p.* 189—254.), there are two miſtakes in tranſlating my words which have given ground to criticiſm. Where I ſay the *os hyoides* is kept firm, or a little brought down, *brought down* is tranſlated *pouſſé en embas*, puſhed down; whereas the evident meaning in French is *abaiſſé* or *tiré en embas.*—Inſtead of tranſlating the Engliſh words, *is the only ſuppoſition to be allowed*, by the French words *eſt la ſeule choſe que je ſuppoſe*, the tranſlation ought to have been *qu'on doit toujours ſuppoſer.*

After Mr Winſlow has mentioned how people may be deceived in making trials of the actions of the muſcles in dead bodies, he blames me (*Mem. p.* 190.—256.) for reclining the head backwards, when I was to examine the digaſtric muſcles, and ſays that I ought to have made the diſſection with the head in the ordinary erect poſture.

In making theſe experiments, I did not free the muſcles from any attachment which could have effect on their actions: I did no more than lay their external ſide ſo far bare as I could take hold of it with a hook, or put a needle into it; and I ſtill think the experiments ought to be made with the head reclined backwards, rather than in any other poſture, for this obvious reaſon, that it is in the reclined poſture only that living people, evidently and without a diſpute, cauſe their lower jaw to deſcend in opening the mouth; and therefore, if the digaſtric muſcles ever have any effect in pulling down the jaw, it muſt be in this attitude. I made the trials in all the poſtures; but mentioned

this one particularly, as being the moſt deciſive of the queſtion in diſpute.

I know not what uſe Mr Winſlow intends ſhould be made of his obſervation, that, in the moſt erect poſture, " the middle curvature of " the digaſtric muſcles, called their *angle* by " Mr Monro, is much leſs than the ordinary " manner of diſſecting has given occaſion to " attribute to them." It is certain that theſe muſcles never are ſtraight in any poſture, and that Mr Monro never gave the name of *angle* to their curvature; and, in the erect poſture, I ſee nothing in theſe muſcles to perſuade me, they are fit for pulling down the jaw, unleſs the experiment is made, as Mr Winſlow has ſometimes done, after taking away the adheſion of the middle tendon of a digaſtric muſcle to the *os hyoides*, (*Mem. p.* 180.—242.)

I am alſo at a loſs to know, what advantage Mr Winſlow can have in the preſent argument, by conſidering the lower jaw as a crooked lever, as he has done (p. 185. & 191. 4to, 249. 257. 12mo): For a power acting on or drawing one end of a beam, the other end of which is the centre of motion, has the ſame force or effect, whether the beam is ſtraight or crooked, provided the power acts in both with the ſame angle of inſertion, and ſame diſtance from the centre of motion.

It is acknowledged (p. 192.—258.), that, when the head is reclined back, and the *os hyoides* is kept firm, " it is certain the poſterior heads " of the digaſtric muſcles cannot act, and conſe- " quently can do nothing for the motion of " the lower jaw:" But then, as the keeping firm

firm or bringing down, or, as Mr Winſlow is pleaſed to quote me, the *puſhing down*, the lower jaw, is ſaid to be only a ſuppoſition which is not to be found in a natural ſtate, as is afterwards to be demonſtrated, the concluſion which might otherwiſe be drawn cannot, it is ſaid, be made.

There is no ſuch demonſtration, as Mr Winſlow here promiſes, to be met with in this *memoire*, and the keeping firm or bringing down the *os hyoides*, when the mouth is opening, is ſo far from an imaginary ſuppoſition, that every perſon, putting a finger on the *os hyoides* while he opens his mouth, can be certain of its being a conſtant evident fact; and therefore Mr Winſlow himſelf is here excluding the poſterior heads of the digaſtric muſcles from any ſhare in the motion of the lower jaw downwards.

Unleſs the keeping the *os hyoides* down is again mentioned as an objection to my 5th ſection, there is none made (p. 192.—258.).

When Mr Winſlow knows, that, in the original of my 6th objection, the perſon endeavouring to feel whether the anterior heads of the digaſtric muſcles are in action, is deſired to do it while the mouth is opened ever ſo wide, quickly, or *ſtrongly*, which laſt word is omitted in his quotation of this objection, he will wiſh ſome of his reflections, which depend on his omiſſion of this word, had not been inſerted.

It is admitted (p. 195.—262.), that the action of other muſcles beſides the digaſtric, is to be felt while the mouth is opened; and (p. 196.—264.) that there is a difficulty in feeling the action of the digaſtric muſcles, though, it is alledged,

it may be felt when the points of the two thumbs are put at the internal or poſterior ſide of the bone of the chin, and reſiſtance is made by the thumbs to the depreſſion of the jaw.

In ſwallowing, any perſon may feel the anterior heads of the digaſtric muſcles in action, without having accurate knowledge in anatomy: I deſire only a comparison to be made, between the feeling of theſe muſcles when even the leaſt forcible deglutition is performed, and when the moſt ſtraining effort is made to open the mouth. In deglutition theſe muſcles are tenſe, hard, and ſwelled; in opening the mouth, their action cannot be diſtinguiſhed: They then feel no more tenſe than the traction of the *os hyoides*, then drawn downwards, would make them, nor more protruded outwards than the ſwelling and riſing outwards of the muſcles above them could cauſe.

In proof the poſterior heads of the digaſtric muſcles acting in opening the mouth, it is ſaid (p. 196---265.), that, "when the fore-finger "is puſhed between the lower part of the maſ-"toid proceſs and the angle of the lower jaw "(as alſo a little more forwards), and then the "finger is preſſed ſtrongly, while the jaw is "drawn down with effort to the laſt degree, "the belly of the digaſtric muſcle of that ſide "may be felt very diſtinctly as a kind of ſtretch-"ed cord, eſpecially, if at the ſame time the "chin is born up by the thumb of the other "hand, to reſiſt the effort of drawing down "the jaw."

I did not expect Mr Winſlow would have inſiſted on this poſterior head acting in opening the

the mouth, after acknowledging it certain that, "if the *os hyoides* is kept firm, (which it moſt "undoubtedly is) this poſterior head can do no-"thing for opening the mouth in the reclined "or extended attitude of the head, in which the "mouth is only opened by the deſcent of the "lower jaw;" and when there cannot be ſo much as a pretence for the favourite baſcule, or reverſion of the upper part of the head upon the condyles of the maxilla. Since, however, he appeals to this trial, I muſt obſerve, that, in making it, no tenſe cord is felt in ſome people; in many it is felt, but in theſe it is very different from what is felt when deglutition is performed, both in its form and ſituation. In this latter action, one can diſtinguiſh the tenſion of the tendon, and the ſwelling of the belly of the muſcle: In opening the mouth, the cord is higher, deeper, and without any perceivable ſwelling in one part more then another; and, therefore, I underſtand it to be the ligament extended from the ſtyloid proceſs of the temporal bone to the *os hyoides* *, ſuſtaining the *os hyoides*, or that it is the ſtylo-hyoideus muſcle performing that office, when there is no ſuch ligament, as frequently is the caſe †.

The laſt argument in the *memoire* for the digaſtric muſcles drawing down the lower jaw in opening the mouth, is (p. 182.--245 p. 191 ---257. p. 197--265.) the diſſection of quadrupeds and birds, in whom the muſcles, analogous to the human digaſtric, are not connected to

* Ruyſch adverſ. dec. 3. § 9. Cowper Myot. in fol. Tab. 24. c. Tab. 27. Fig. 1. DD.

† Weitbrecht Syndeſmolog. Sect. vi. § 15.

to the *os hyoides*, but are extended in a ſtraight courſe to the lower jaw, to the middle or back part of each ſide of which they are fixed, and evidently draw it down.

I agree with Mr Winſlow both in the anatomy and uſes of theſe muſcles in quadrupeds and birds, but will not admit his concluſion, that the human digaſtric muſcles have the ſame office; for theſe muſcles of theſe creatures have not the curvature in the middle nor the connection to the *os hyoides*, nor the inſertion into the chin, which the human digaſtric muſcles have, and which are the circumſtances upon which my objections to the common account of the action of theſe human muſcles are founded.---If any perſon was to argue thus: The human digaſtric muſcles are principal inſtruments in deglutition; therefore the analogous muſcles of quadrupeds and birds are alſo organs of deglutition, Mr Winſlow would refuſe this concluſion to be juſt, for the ſame reaſons as make me not to admit his concluſion.

After objecting to the common doctrine concerning the digaſtric muſcles, I took notice of both jaws contributing to the opening of the mouth; the upper part of the head only moving when the chin is near the breaſt; the lower jaw only moving while the head is extended fully back, and both jaws moving in all the intermediate ſituations of the head, each of them moving nearly proportionally to the greater or leſſer flexion or extenſion of the head. In proof of this, I deſire any perſon to put a knife, or his nail, oppoſite to the conjoined edges of the teeth, before the mouth is opened, to hold the

knife or nail unmoved, while, ſtanding before a mirror, he opened his mouth in all theſe different attitudes of the head, and then he might ſee theſe motions of the jaws.

In the reflection on theſe experiments (*Mem. p.* 198—267.), Mr Winſlow ſeems to think that, "when there is not ſpace enough to make a "large opening of the mouth by the deſcent of "the lower jaw, one is naturally prompted to "raiſe or recline the head backwards proportion-"ally to the extent of ſpace neceſſary to make "the opening of the mouth by the ordinary de-"preſſion of the lower jaw; and that it is thus "that crocodiles raiſe the head to make a large "throat, according to the obſervations on three "crocodiles related in the collection of old *me-"moires.*"

If, in opening the mouth, while the chin is near the breaſt, all the head was firſt raiſed, and then the lower jaw deſcended, there would be ſome reaſon for this reflection; but, as the lower jaw is not in the leaſt raiſed, but keeps its ſituation, while the upper part of the head only moves, it cannot invalidate what I have ſaid; and there cannot be any intention of making ſpace for the deſcent of the lower jaw, when this jaw is not to deſcend.—The Academicians, whom Mr Winſlow quotes, evidently thought of crocodiles raiſing their upper jaw and cranium by the muſcles placed on the back part of the vertebræ of their back and neck.

In the ſame reflection (*p.* 199—268.) it is ſaid, that, in the experiment I propoſed for ſeeing the motion of the jaws in a mirror, the perſon extends his head, that he may look into his throat: —Whereas

——Whereas the experiment ſucceeds, whether the perſon ſees or is blind, whether he knows for what purpoſe it is made, or is ignorant of it, whether he holds the knife himſelf, or another does it without his knowledge. In ſhort, whoever makes the trials, or ſees another do them with a mirror, or without one, muſt be convinced I have deſcribed what really is done in opening the mouth.

I am ſurpriſed the reflections above mentioned were inſiſted on, ſince they contradict the baſcule, or reverſion of the upper part of the head upon the condyles of the lower jaw, argued for (*p.* 181—243. *p.* 184—247.); which I would next examine, if it was not more proper to conſider it in my remarks on Mr Ferrein's *memoire*, where it is more fully treated than in Mr Winſlow's.

Whether the uſes I have mentioned of the muſcles below the *os hyoides*, to wit, the ſterno-hyoidei, coraco-hyoidei, &c. are aſſented to or denied, (*p.* 195——262.) I cannot ſay; for tho' it is admitted, that "in great efforts to bring the "lower jaw down to the laſt degree, and by this "to open the mouth greatly, the tenſion, and a "kind of hardneſs from this tenſion of theſe "muſcles are felt, yet it is ſaid, that one ſhall "not diſtinguiſh any hardneſs by the contraction "of their fleſhy fibres, nor any ſhortening by "that contraction."

Any perſon putting his finger on the ſkin which covers theſe muſcles, while he moves his lower jaw, can determine as well as the moſt accurate anatomiſt what theſe muſcles then do; and therefore I ſhall appeal to every one's feeling for

determining this part of the debate. My colleague Dr Rutherford, Mr Gibſon ſurgeon at Leith, and I, were conſulted about a tumor in the thyroid gland of a very lean lady, in whom the coraco-hyoid, ſterno-hyoid, and ſterno-thyroid muſcles raiſed the ſkin covering them, on even the leaſt motion made by her lower jaw in ſpeaking to us, and then felt as firm hard cords under our fingers.

After Mr Winſlow has finiſhed his reflections on Art. xi. of your vol. 1. he criticizes the foregoing ſupplement, but has nothing new in his remarks, except blaming me, (p. 202—272.) for making uſe of figures of the parts put in a wrong attitude, and particularly the one I had cauſed to be drawn of theſe parts, as they appear when the head is reclined back.

I have already ſaid, and beg leave to repeat, that this is the moſt proper attitude in which the digaſtric muſcles can be examined, in order to paſs judgment on the diſpute between us; becauſe it is in this poſture, that the opening of the mouth is ſolely made by the deſcent of the lower jaw; and therefore, if the digaſtric muſcles are the organs which pull down this bone, then it is that their action muſt be greateſt, and conſely moſt manifeſt.

I ſhould have thought myſelf very happy in being prevented from cauſing any original anatomical figures to be drawn, which I probably would have been, had Mr Winſlow given us the figures of all the parts of the body, as he intended; for, had the painters and engravers executed their parts with the ſame accuracy and candour, as he would have diſſected and preſented

 the

the originals to them, we ſhould have had by far the moſt compleat ſyſtem of anatomy ever yet has appeared, though, as frailties muſt attend human nature, even a Winſlow might have erred. Long may he live to do ſtill more ſervice to mankind; but I am afraid his great age will not bear the fatigue of ſuch laborious work as theſe plates would require.

## Anſwer to Mr FERREIN.

THE Academy of Sciences at Paris has publiſhed two *memoires* of Mr Ferrein, relating to the motions of the jaws, in their Tranſactions for the year 1744. In the firſt of which there is (*p.* 427. &c.——579. &c.) a deſcription of the articulation of the lower jaw, agreeing with the one in Art. xi. of your vol. I.; except that 1. (*p.* 427———579.) the ſubſtance interpoſed between the two pieces of which the lower jaw conſiſts in infants, and between moſt other bones of the head, ought, it is ſaid, not to be called a cartilage, as is commonly done, but a continuation of the perioſteum.——2. (*p.* 329———580.) The ſubſtance which lines the cavity that receives the condyle of the lower jaw, is likewiſe ſaid to be no cartilage, but a ſimple membrane.———3. (*p.* 430———582.) The elaſtic *meniſcoid interarticular* cartilages are affirmed to be ligamentous bodies.——4. (*p.* 430 ——582.) The external and internal parts of the articular ligaments, which are thicker than the fore and back parts of theſe ligaments, according

ing to the common rule, of the ligaments of the articulations by ginglimus being thicker at the fides to which leaft motion is allowed, are here efteemed four ligamentous cords.

Seeing felf-defence alone obliges me to take up the pen at prefent, and none of thefe four fpecialities are particularly defigned to criticize me, I fhall enter into no difpute about them.

The motion of the jaw forwards, or its horizontal motion, is defcribed in the *Memoire*, (*p.* 431—584.), in the fame manner as in your Effays; but (*p.* 431—584. *p.* 432—586.) it is afferted, that authors have expreffed themfelves fo obfcurely concerning the motions of the jaw backwards, that it was impoffible to know what they thought.

Whether is it impoffible to underftand the following words, (Edin. Eff. vol. 1 *p.* 104.) "The exterior extremity of this oblong pofte-" rior cavity is made very narrow by the *mea-* "*tus auditorius externus* being confiderably ad-" vanced forwards here, which again prevents "the condyle ever to be pufhed fo far back as "to be in hazard of doing any injury to the "gland fituated in the more internal part of "the cavity?" Whether is not this a more particular account of the non-receffion of the lower jaw, than to fay in general, as is done in Memoires 1744, (*p.* 433——586.) that it is owing to the conftruction of the parts, and the oppofition of the ligamentous cords?

Mr Ferrein, obferving that the condyles are too much ftraitned in the glenoid cavities, for performing the large motions which the lower jaw makes to each fide, (*p.* 434—588.) is aftonifh-

ed, "that an obſervation ſo ſimple, and a "truth ſo ſtriking, has eſcaped the eyes of ſo "many anatomiſts."——Has he not overlooked the following words of the Eſſays, vol. 1. p. 106. "The condyles can be moved laterally when they are lodged in the cavities, or on "the tubercles; but theſe lateral motions are "much more confined in the cavities, becauſe "of the ſurrounding brims?"

Both anatomiſts and ſurgeons knew that the condyles of the lower jaw did not always advance and recede together, but were capable of being moved forwards or backwards, ſeparately or alternately. They are, however, obliged to Mr Ferrein for his minute deſcription of theſe motions, and their effects on the different parts of the jaw, (*p.* 435—588. *p.* 443—599.)

In *p.* 439.——594. Mr Ferrein has made a very pretty diſcovery, and has corrected an error into which all the anatomiſts had fallen, concerning the manner of the motion of the lower jaw in its depreſſion and elevation.—— The condyles had always been thought the axes upon which the lower jaw moved, as moſt other bones do upon their articulated ends; but Mr Ferrein has demonſtrated the axis of motion of the lower jaw, in its motions upwards and downwards, to be a little above its angles; ſo that, when the body of the bone moves downwards, its condyles advance forwards, and when its body is raiſed up, the condyles recede back; by which, ſays he, the parotid glands are ſafe from compreſſion, as the muſcles which raiſe the jaw are from being overſtretched; and it may be added, that the nerves and blood-veſſels which

which enter the internal posterior hole of the jaw, are likewise saved from being twisted, over-stretched, or torn, in the motions of this bone. I shall conclude my remarks on this first *Memoire* of Mr Ferrein, by observing that this account of the aperture of the mouth and experience confirm what I have said, (*Ess. vol.* I. *art.* xi. *p.* 108.) of the danger of a luxation of the lower jaw when the mouth is opened, and the condyles are on the tubercles of the temporal bones. It is in vomiting, coughing, yawning, &c. that these luxations ever happen, so that he should not have quoted this opinion as erroneous, (*p.* 444—602.).

THE first thing Mr Ferrein undertakes to prove, in his second *Memoire*, is, (*p.* 511—687.) "that the head contributes to make the opening "of the mouth, and that it even contri-"butes considerably, but by a motion indepen-"dent of the will, and of the contraction of "the splenius, complexus, and other muscles "which raise the head; in a word by a mo-"tion reciprocal to that of the lower jaw, re-"sulting from the effort that is made to de-"press the lower jaw, and occasioned by the "resistance which it opposes to its depression."

I suspect I don't rightly understand this proposition; for I am at a loss to know what motion can be independent of the will, which results from an effort, and which is said, (*p.* 540—729.) to be an effect of the act of the will; and I am not certain whether the reciprocal motion here mentioned is Mr Winslow's bascule, or reversion of the head upon th

condyles of the lower jaw. I ſuppoſe it is; and therefore am now to perform the promiſe I made in my anſwer to him.

As an introduction to Mr Ferr[illegible] [illegible]on-ſtration of the foregoing doctri[illegible]e, he mentions the following law in mechanics: "I ſuppoſe "two bodies A and B, faſtened to the two ends of "a cord which ſhortens itſelf; it is evident that "the cord draws theſe two bodies, the one to-"wards the other, in making them paſs over "ſpaces reciprocal to their maſſes, and that if "the maſs A is double or triple of the maſs "B, the ſpace which the maſs B moves ſhall "be double or triple of what A moves."

If the force with which the cord ſhottens itſelf is leſs than the reſiſtance of the maſs A, how much ſpace would that Maſs A move? None ſurely. This is the caſe of muſcles, which almoſt perpetually act with a force which moves only the more moveable of the two parts to which their ends are connected. In proof of this I ſhall take Mr Ferrein's own examples of the fore arm and leg, which he has brought (*p.* 512—688.) to prove his mechanic law. ——When the leg or the fore arm alone is to be extended, its muſcles move it only, ſo far as according to the rule I juſt now ſaid is generally obſerved; but, ſays Mr Ferrein, the reaſon why the thigh or arm don't move at the ſame time, is the reſiſtance made to the motion of the thigh and arm by the almoſt enormous maſs of the trunk, &c. which muſt be moved with them.—Let us ſuppoſe this a good reaſon, then obſerve what happens when one riſes from a chair, if it is even on one foot. The exten-

ſor

for muscles of that leg raise the enormous mass above it; and leave the leg immoveable; and in the same way one having his hands on a table with his fore-arms bended, can raise his body from the ground by the action of the extensors of the fore-arms, raising the arms and all the enormous weight of the body, the fore-arms remaining unmoved. If Mr Ferrein's law obtained in the action of muscles, and if a leg and foot, or the two fore-arms and hands, are one tenth or eighth of the weight of the whole body: then, in these cases of rising from a chair, or raising our weight on our hands, the leg or fore-arms ought to move only one tenth or eighth less space than the thigh or arms move; whereas they move none. Thus muscles move only the least resisting part of the two they are connected to, though the difference of the resistance is but one eight or tenth; nay, if a man rises with a weight in his arms, or on his back, the difference may not be one twentieth or thirtieth.

After assuming this mechanic law of muscles moving both the parts they are connected to, it is inferred, (p. 512——689.) that "the "principles which are established, are certainly "sufficient to demonstrate, that the effort which "one makes to open the mouth ought to tend "at the same time to depress the lower jaw, and "to raise the upper one."

Admitting the law in mechanics to have been demonstrated, and justly applied to the action of muscles, and granting every thing yet mentioned in this *memoire*, this consequence would not follow; because there has yet been no proof

proof brought of a contractile cord extended from the head to the lower jaw.

In applying the law we have been disputing about, Mr Ferrein says, (p. 513—680.) "I "have discovered, that, notwithstanding the "head's being nearer the trunk than the lower "jaw, yet the resistance which the head makes "to its elevation is extremely little; and such "nearly as it would be, if the head was wholly separated from the spine. I have also "found, that the resistance of the lower jaw, "tho' really less, (meaning, I suppose, than that "of the head), is however considerable enough "to occasion in the head the motion which I "have attributed to it," to wit, the elevation.

One must read the subsequent page of the *memoire*, before he can understand what the resistance of the head wholly separated from the spine is, which is here intended. It is, I suppose, the resistance to elevation which the head makes, when its upper part is made to move on the condyles of the lower jaw.

The heads of people falling asleep, or of children, and of weak persons, when they are in an erect posture, bend down their breasts, and whoever raises the head of a man in a faint, may find, that, in doing it, he must employ a considerable force, so that the resistance to the elevation of the head is not extremely little; and seeing it is here acknowledged, that the resistance of the lower jaw is less than that of the head, it will be concluded, from the foregoing experiments with the legs and fore-arms, and many more such might have been named, that, supposing a contractile cord to be extended from the

the back part of the head to the lower jaw, this cord would only move the lower jaw, which is confeffed to give the leffer refiftance.

In the fubfequent paragraph, there is an acknowledgment, which I heartily wifh had been more attended to. The words of it are, "In "truth, the reciprocal motion of the head up- "on the lower jaw cannot be made, if the pro- "ceffes articulated with the fpine (thefe muft "be the condyles of the occipital bone) don't "follow the determination which the motion "of the head gives them."

The condyles of the occipital bone are the fulcra on which the head refts, and on which the motions of the cranium and upper jaw, independent of the fpine and lower extremities, muft always be performed. Whoever affirms, that the condyles of the lower jaw can be the fulcra and axis of thefe motions, might as well fay, that a beam, AB, placed on the rigid pillar C, could ever have its axis of motion on the end D of another beam, DE, joined to AB by a hinge, where, it is evident, that, on raifing the end, A, upon the axis, D, the other end, B, of the beam, muft make its way down thro' the fubftance of the pillar, C, as the prick'd line, *ab*, does in Fig. 1. :——Or, if we would fuppofe, as in Fig. 2, that the beam, FG, fupported by the pillar, H, is to have its end, F, moved downwards upon the axis, I, the end of a beam, KI, fixed to FG by a hinge; then it is evident, that FG muft rife greatly from the end of the pillar, as the pricked line *fg* does in the figure. It is almoft needlefs to add, that the cranium, and upper jaw, are here reprefented by the lines AB,

AB, *ab*, FG, *fg*. The pillars, C and H, are instead of the spine, and the crooked lines, DE and IK, are in place of the lower jaw; on the supposition of whose condyles being the axes of motion of the head, the spine must be immensely compressible and elastic, to be lengthened and shortened so greatly, in the opening and shutting the mouth.

'Tis in vain to endeavour to elude the force of this objection to the reversion of the head upon the condyles of the lower jaw, by saying, as Mr Ferrein does, p. 514—690), That "the "motion of the head on the first vertebra of "the neck, and on the lower jaw, has nearly "the same axis."——Mr Ferrein knows very well, how much farther back the condyles of the occiput are, than either the glenoid cavities, or tubercles of the temporal bones, with which the lower jaw is articulated; for, of the space between the occipital condyles and the fore-part of the jaws, the distance between these condyles and the middle of each cavity at the back part of the tubercle is about one fifth; and the distance between these condyles and the middle of the tubercle, is more than one fourth: But, if the occipital condyles are at all behind these parts of the temporal bones, it is absolutely impossible, that, while the head and spine are kept together, the fulcrum, or axis of the head can be on the condyles of the lower jaw, or indeed any where, but on the occipital condyles or spine: And Mr Ferrein seems conscious of this; for he does not affirm, that the axes are precisely the same, which they ought to be, in his and Mr Winslow's opinion of the motions

ons being performed on the condyles of the lower jaw, but only affirms here and afterwards, (p. 517.—695.), that they are nearly the fame.

The only way to make the head move on the lower jaw would be to take away its fupporting pillar the fpine, that the back part of the head might be left loofe and free; and then to hold the lower jaw firm. Upon comparing the axis of motion, in thefe motions performed by the head in this fituation, it may be feen very different from what it is while the fpine fupports the head, whether the vertebræ are put on a table, as Mr Ferrein (p. 514.—690.) propofes, or whether no part of the fpine is feparated from the body, which is much the better way of making this experiment: The axis of motion of the head is therefore the fame, whether it moves with or without the opening of the mouth.

Mr Ferrein, aware that, though all his preliminary propofitions were granted, yet the lower jaw muft make a confiderable refiftance before the head could be raifed, mentions, (p. 515.—692.) four caufes which confpire to make this refiftance, to wit, 1. The inertia of the parts, which, as in all matter, makes them refift motion.—2. The *ployement* of the parts which compofe the fore-part of the neck.——3. The connections of the lower jaw, with the upper part of the neck.—4. The flefhy body of the pharynx, which is connected to the neck and lower jaw.

I am furprifed that the firft of thefe caufes is mentioned to prevail in a pendulous member, whofe gravity is making it tend downwards.—I did not tranflate *ployement* in the account of the fecond caufe, becaufe I cannot think this word

word is here taken in the common ſignificatio of bending, ſinking, ſhrivelling, or ſhortening which would all contribute to bring down the lower jaw; and, if I was to take any notice of the 1ſt, or of this 2d, and the 3d and 4th cauſes, it would have been to enumerate them as cauſes which draw the lower jaw down; for they have not only gravity to cauſe them to deſcend, but the muſcular contractile organs, on the fore part of the neck, being connected below to parts which are much more difficultly moved than the lower jaw, to wit, the ſternum, clavicles, ſcapulæ, diaphragm, whatever power of contraction they have muſt be a niſus to draw down the jaw.

It is however needleſs, in the preſent argument, to inſiſt on a minute claſſing of the cauſes, when we every day ſee the jaw falling, and the mouth open, in drunken, paralytic, dying perſons, or whenever the levators of the jaw become weak, nay, whenever the attention of the mind is taken off from the office of ſuſtaining the jaw, it falls. Thus people in profound meditation generally have their mouths open; and a country fellow ſtands gaping, while he is gazing at the novelties of a town. ——When the cheeks and the muſcles, which are levators of the lower jaw, are cut in a dead body, the mouth remains open, while the head is in the erect poſture.

I ſhall make no remarks on Mr Ferrein's proof of the poſſibility and reality of the motion of the upper jaw in opening the mouth (contained in *mem. p.* 515, 516, 517.—693, 694, 695.) but beg the reader to compare them with a few lines in p. 115. of vol. 1. of Edinb. Eſſ.

This

This section of Mr Ferrein's *memoire* is concluded, p. 516.—695, with five remarks.

"1. The axis of motion in the depression of "the lower jaw is, according to him, nearly the "same with the axis in the elevation of the upper jaw.

"2. These axes are both very different from "the axis of the natural motion of the head, "such as is made naturally, and without con-"straint, by means of the splenius and complex-"us."

Of the doctrine in these remarks, enough is already said."

"3. The reciprocal motion of the upper "jaw is independent of the contraction of the "splenius, complexus, and other levator muscles "of the head; this is demonstrated by reason and "experience."

I see no reason for this in the *memoire*, but I shall take one against it from Mr Ferrein's own doctrine, which is using what is commonly called *argumentum ad hominem*. In p. 541.—730. he gives it as a maxim, "That every mus-"cle being connected at its two ends to two dif-"ferent bones, every motion of the one or of "the other of these two bones, which makes one "of these ends to come nearer to the other, de-"pends, at least in part, upon the contraction "of that muscle." Now, whatever axis the upper part of the head has in its elevation, Mr Ferrein will certainly grant, that the back-part of the head descends during the elevation of the fore-part of it; and this back-part cannot descend without the end of the splenius and complexus inserted into it coming nearer to their

other end, which rises from the vertebræ; and therefore, by his own maxim, these muscles, at least in part, serve to bring down the back-part of the head, and thereby raise the fore-part of it.

The experience he appeals to is, to apply the fingers upon the teguments covering the splenius and complexus when the mouth is opening; when, he affirms, these muscles are not felt hard or tense, unless when the separation of the two jaws being come to the last degree, one endeavours to force further the opening of the mouth.

The answer to this experience is, That in every person, blind or seeing, ignorant or acquainted of the intention, the action of these muscle, is always felt in opening the mouth, especially when done quickly, strongly, and with repeated jerks, the person who makes the trial remembering that they are thin broad muscles covered with thick skin and fat, and with the cucullaris muscle; so that they cannot be expected to swell so much outwards, during their contraction, as a masseter, gastrocnemius, or such other thick muscle which is little covered.

"4. The upper and lower jaws concur in the "same manner to open the mouth, since they "divide the motion which produces this effect."

Is not this the doctrine of your essay-writer?

"5. It is probably the same in the crocodile, "as well as in a great number of other ani-"mals: It is known that the crocodile, in open-"ing its throat, raises the upper jaw consider-"ably: We are assured, in the old *memoires* of the

"*academie*,

" *academie*, that this elevation is greater than " the depreſſion of the lower jaw; there ſix " muſcles are deſcribed, which are in place of " the ſplenius, complexus, &c. and which are " there ſaid to produce the motion of the upper " jaw."

This is ſuch an explicite declaration of theſe ſix muſcles ſerving to open the throat, that I can't imagine how it came to be brought as an argument againſt me, unleſs the remark which follows is thought to explain this declaration to the reverſe of its obvious meaning. The remark is, " All the muſcles of which we have " ſpoke, are only appointed to raiſe or bring " down the head; and thus, when it is ſaid " that thoſe which are placed upon the back-" part of the back and neck ſerve to raiſe the " jaw, it is not properly the jaw which is rai-" ſed—but the upper jaw, and the cranium; " for the bones which compoſe theſe two parts " are firmly connected to each other."

In this the Academicians are guarding againſt their readers believing, that they joined in the common opinion which prevailed at that time, of a crocodile's upper jaw having a moveable articulation, and moving independent of the motions of the cranium, in the ſame way as the upper beaks of parrots move, without any intention of contradicting what they had ſaid of the muſcles analogous to the ſplenius and complexus raiſing the upper jaw.——Nay, Mr Ferrein's introducing his explication of the words of the Academicians with, *apparemment*, and afterwards with *il eſt a preſumer*, *probably*, and *it is to be preſumed*, ſhews that he was not con-

vinced his explication was just, but rather that he gave it in complaisance to Mr Winslow.

In the second article or section of the *Memoire* I am examining, Mr Ferrein treats of the muscles which produce the motions of the one or the other jaw; in which, to shun digressions, he premises some preliminary observations, which I shall consider in the same order he has put them.

1. (*p.* 519—698.) "In polygastric muscles, the "contraction of one belly is independent of the "contraction of another."

I agree to this, but don't chuse to adopt all the reasoning in proof of it; which, however, not relating to the subject in dispute, I shall pass without further notice.

2. (*p.* 521—701.) "The inflexion of the "tendon of the digastric muscle is produced by "an annular ligament, and by an aponeurosis "which is fixed to the *os hyoides*."

I have described and painted the aponeurosis. Cowper * mentioned an annular ligament; and it is painted by Courcelles †, who says, "in omnibus subjectis non tam distincte conspicitur "annulus;" and Mr Ferrein admits, that it is inconstant in its appearance, varying in its thickness and breadth, and being (*p.* 529—712.) frequently enough, and (*p.* 531—715.) very often, intirely wanting.

In the comment on this second observ. (*p.* 513, —783.), there is a little mistake committed, when it is said, the 13th article of your vol. III. is accompanied with two figures of Cowper, there being

* Anat. explicat. of tab. 15. et Myot.
† Icon. Musc. capit. 4. Lugd. Bat. 1743. tab. 4.

being but one figure of Cowper's. And, immediately after, by beginning a quotation from that article in the middle of a sentence, I am made to say what I never intended, to wit, that this ring of his is nothing but a cellular membrane; whereas, by beginning the quotation five lines higher, or reading the 3d objection in art. XI. of vol. I. every one may see, that I denied such a pulley to the tendon of the digastric muscle, as the superior oblique muscle of the eye, or the muscles of the fingers, have to play in; unless the cellular membrane had been mistaken for a pulley. Mr Ferrein agrees with me, that there is no such pulley, tho' he is pleased to give the name of annular ligament to some fibres of the aponeurosis, which, rising in greater or smaller number in some subjects, must pass over the tendon, to join others, which are extended from the inside of that tendon.—Whether do such uncertain fibres warrant one to lay down a general maxim, "That "the inflexion of the tendon of the digastric "muscle is produced by an annular ligament," especially, seeing the inflexion is always made, whether there is or is not such a ligament?

The third and fourth observations in the *Memoire* being approbations of the doctrine in your essays, concerning the curvature, or tendon of the digastric, which cannot be raised without the *os hyoides*, and its want of a pulley, I shall make no remarks on the explications of them, that I may avoid more dispute than is necessary for my own defence.

Mr Ferrein, observing some fibres of the stylo-hyoid muscles joined with the digastric, con-

cludes, in his 5th obfervation, (*p.* 531—715.) that "the ftylo-hyoideus mufcle, and the pofterior "belly of the digaftric, may be confidered as "one and the fame mufcle with two heads."

If the connexion of the aponeurotic fibres of mufcles fhould give the name of *digaftric, trigaftric*, &c. to all the mufcles which have fuch connexion, the number of the many bellied mufcles muft be greatly increafed.

(P. 535—720.) It is faid, "the ftylo-hyoideus, "and pofterior belly of the digaftric, are appointed to act together, and to produce nearly "the fame effects."—How does this agree with the firft obfervation, (*p.* 515—648.) "In polygaftric mufcles the contraction of one belly "is independent of the contraction of another?"

The 6th obferv. is (*p.* 335—721.) "The "*os hyoides* departing from its natural fituation "cannot be moved from before backwards." The reafons for which is faid to be, the ends of the cornua of the *os hyoides* then refting upon, or being fupported by the tendinous parts of the *mufculi recti anteriores majores* of the head, which are fituated immediately upon the vertebræ.

One would think that, as mufcles have a conftant nifus to fhorten themfelves, the curve which the mufcles above and below the *os hyoides* make, was intended to keep the points of its cornua from refting on the vertebræ, to prevent the parts there from being hurt by the preffure of thefe points. And, when one is directing no effort in thefe mufcles, but they are left to their natural action, a finger on the

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base of the *os hyoides* can push it back, and, when we observe the motion of this bone in deglutition we see it recede farther back than its natural situation.

The 7th observation is to confirm what I had said concerning the keeping firm or bringing down the *os hyoides* in opening the mouth. But, in the account of the dispute about the digastric muscles, the word *uniquement*, p. 537—723. and the word *seul* (p. 550—743.) ought not to have been put; for I never said that the digastric served only for deglutition.

I am here in an unlucky situation between the Academicians. Mr Winslow, as I observed formerly, blames me for considering other actions of the two heads of the digastric muscles than the simultaneous one; and here Mr Ferrein blames me as much for considering only the simultaneous one. Whoever reads your essays will find Mr Winslow has understood me best.

After what I have said concerning the impossibility of the condyles of the lower jaw ever being the fulcra or axis of the head in its motions, it is needless to shew Mr Ferrein's misapplication of his observation of the maxillary condyles advancing forwards in opening the mouth, which (p. 538—725.) he would have us to believe is the head shuffling back. He will see the mistake by only trying whether he feels this shuffling backwards in the mastoid process, and whether the ligaments which join the occipital condyles to the vertebræ are long enough to allow this shuffling back of the head?

The use of the external pterygoid muscles, in bringing forwards the condyles and meniscoid cartilages

cartilages of the lower jaw, is next defcribed in nearly the fame manner as in your effays; and (p. 539—727.) the fame action of bringing down the lower jaw is afcribed to the mylo and genio-hyoidei mufcles; but, in proof of this laft propofition, I am made to fay, that the *os hyoides* moves towards the lower jaw, when the jaw is brought down; or at leaft I muft be inferred to fay fo from my afferting, that the mufcles above and below the *os hyoides*, having a confiderable curve at this bone, muft draw it forwards when they are acting.

If I had not faid that the *os hyoides* is brought down and reftrained from coming too far forwards in opening the mouth, there might have been fome grounds for the inference above mentioned; but, as I affirm both thofe facts, the inference ought not to have been made from one circumftance, without mentioning the other two.

The fecond propofition (p. 540—729.) is, "The ftylo-hyoideus mufcle ferves to raife the "upper jaw;" the proof of which is the motion of the two jaws upon the fame axis; the motion of the upper jaw very different from the natural motion of the head, and altogether independent of the contraction of the fplenius, complexus, or its other elevators; and therefore, the raifing of the upper jaw muft be by the action of the ftylo-hyoideus and pofterior head of the digaftric.

If your readers are convinced, that the circumftances in the antecedent propofition are difproved, as I have endeavoured to do, they will reject the confequence here drawn.

Tho' we follow Mr Ferrein in his fuppofition

(p. 541

(p. 541—729) of the ſtylo-hyoideus muſcle being extended to the chin inſtead of being fixed to the *os hyoides*, we would not find this muſcle could make any effort to raiſe the upper jaw: For the head always performing its motions on the ſpine, and the ſtyloid proceſs, which is the part of the head to which this muſcle is fixed, being farther forward than the axis of the head's motion, the muſcle never can draw down the back-part of the head to raiſe the fore-part.

The 1ſt proof the ſtylo-hyoideus raiſing the upper jaw is founded on the following general rule: "Every muſcle being connected at its " two ends to two different bones, all motion " of one or the other of theſe bones, which " cauſes one of theſe ends to come nearer to " the other end, depends, at leaſt in part, on " the contraction of that muſcle. The rule, " it is added, is perhaps new."

Is it new to you, gentlemen, that muſcles ſhorten, or endeavour to ſhorten themſelves, when they act, and that they muſt be ſhorter when their ends come nearer each other? And, if the rule is true, does it not prove what I formerly inferred from it concerning the action of the ſplenius and complexus?

Whoever lays it down as an univerſal principle, that every muſcle becomes ſhorter when it acts, or that every muſcle which ſhortens itſelf, is exerting its force, is in a miſtake, as is evident in numerous examples. Thus, when one ſits down on a ſeat, the extenſor muſcles of the legs, and the retractors of the thighs are in ſtrong action, yet are lengthened, while the flexors

flexors of the legs and thighs are ſhortened, but unactive.——The extenſors of the ſpine are the acting but elongated muſcles, when we bow forwards. Nay, of two muſcles which ſeem to have nearly the ſame direction and uſe, the one may act without the other. Thus the *biceps flexor cubiti* and *brachiæus internus*, generally act together in bending the fore-arm; but, if the fore-arm is to remain extended, or is to be kept fixed in any degree of flexion, while the arm is to be ſtrongly raiſed and drawn forwards, the biceps contracts ſtrongly, and the brachiæus is unactive; or if the fore-arm is to perform ſtrongly the action of pronation, while it is to be bended with little force, the brachiæus may act while the biceps is unactive, leſt this latter muſcle counteract the pronators, whoſe force is then principally required.

I hope the feeble criſpation of the muſcles which I have ſaid to be unactive in the foregoing caſes will not be called their action, in order to find a contradiction in my words. Every one ſurely knows the difference between the weak natural curtation of muſcles and their ſtrong voluntary contraction.

If the rule is faulty, the example of the ſtylo-hyoideus, as here applied to illuſtrate it, is one of the moſt unfavourable in the body, becauſe, this muſcle being fixed to a part of the head farther forward than its axis of motion, it is impoſſible ſuch a muſcle ever can raiſe the head.—But, ſuppoſing the connection of this muſcle to the head to be farther back than its axis, and ſuppoſing the cornua of the *os hyoides* to reſt with their

their points on the vertebræ of the neck, can it be imagined that ſuch ſlippery round points could poſſibly ſtand ſo firm on the ſlippery vertebræ as to be a fulcrum to the head in its elevation, eſpecially too while the traction is at the other end of the cornua, and in a very oblique direction? or could ſuch points be preſſed with ſo much force upon the muſcles covering the vertebræ without hurting them?

In my anſwer to Mr Winſlow, I mentioned what I thought the uſe of the ſtylo-hyoideus muſcle in opening the mouth; and therefore may paſs Mr Ferrein's ſecond propoſition, which treats of that ſubject, without examination, as I may alſo do to his third propoſition, wherein he aſſigns the ſame uſe to the *mylo*, *genio*, *ſterno*, *coraco hyoidei* muſcles as I had done.

In his fourth propoſition, (*p.* 543—732.) he undertakes to prove "that the mylo and genio-"hyoidei aſſiſt in certain caſes the ſtylo-hyoi-"dei to raiſe the upper jaw, but that the ſtylo-"hyoidei never can aſſiſt the former to bring down "the lower jaw."

The power of the ſtylo-hoidei to raiſe the upper jaw, and the cornua of the *os hyoides* ſerving as fulcra to the head in its elevation, being the foundations on which the proof of this propoſition is laid, I need not do more than mention it, and refer to what has been ſaid againſt theſe foundations of it, which may alſo ſerve as an anſwer to what is ſaid of the poſterior head of the digaſtric muſcle (*p.* 544—545—734—736.) where the ſame principles are made uſe of.

What is ſaid (*p.* 544—545—734—735.) of the anterior head of the digaſtric, is appealing to his general

general principle about the ſhortning of muſcles, and repeating what Mr Winſlow had ſaid, which, being already examined, need not now be conſidered.

Had not Meſſrs Winſlow and Ferrein been ſo fond of the impoſſible reverſion of the head, upon the condyles of the lower jaw, they might have had a better argument than any they have employed for the action of the digaſtric muſcle in opening the mouth: They might have obſerved, that the poſterior belly of the digaſtric is fixed to the ſkull, at leaſt as far back as the condyles of the occiput, where the axis of the motion of the head is; and therefore, when that muſcle acted, it might draw down the back-part of the head, and ſo raiſe the fore-part, while the anterior belly brought the lower jaw down.

Leſt ſome other make uſe of this argument againſt me, I muſt obſerve, that the axis of the head is much farther back than its center of gravity, on which account the head falls forward on the breaſt, whenever the organs that ſupport it are weak and unactive, as in infants, ſleeping, drunken, fainting, apopletic people, whenever they are put in the erect poſture. The organs which ſupport the head from falling forwards are the ſplenius, complexus, and other muſcles placed on the back-part of the neck, as is evident from the head's bending forward when theſe muſcles are cut tranſverſely, from the retraction of the head when they are convulſed or inflamed; and many more ſuch proofs might be named. Theſe muſcles are not only ſtrong by their number of fibres, but they are inſerted into

to the head in a much more advantageous way, than moſt muſcles of the body are in their reſpective bones; for they are fixed into the head nearly at right angles, and at a conſiderable diſtance from the axis, that they may have a long lever to act with; and their other end is fixed to the vertebræ, where little motion in the direction of their traction can be allowed. Whereas the digaſtric muſcles have few fibres, are fixed very near the axis, at a very acute angle, and are connected at their other end to a very looſe moveable part; and therefore can never officiate inſtead of the other powerful organs. Nor would the fixing of the *os hyoides* by the muſcles below it render the digaſtric any way equal to the office of raiſing the head, while the action of the poſterior head of the digaſtric would diminiſh the force by which the lower jaw is drawn down juſt as much as the power of its own action: For it muſt be evident that the lower jaw can be drawn down with no greater force by muſcles fixed to the *os hyoides*, than what is exerted to keep that bone down. Whenever the power is greater that draws the *os hyoides* up, than what pulls it down, it muſt be moved towards the jaw, which it never is in opening the mouth.

*P.* 547. 548.—739. It is granted, according to what is in your eſſays, *p.* 117. that the mylo and genio-hyodei muſcles draw the jaw backwards.

I paſs ſeveral little things which may be criticized in the four following pages, to come to the concluſion of this memoire, where it is aſſerted, "That Bidloo, Cowper, and Monro conclude it "impoſſible to ſwallow, while the mouth is o-

"pen; which reafoning is deftroyed by expe-"
"rience, as may be obferved in thofe who"
"drink, pouring the liquor from above, into the"
"mouth."

I can fee no fuch affertion in Bidloo, either where, in *tab.* xv. of his *Anat. corp. hum.* he has painted the digaftric mufcle, nor where the lower jaw is delineated in *tab.* xcii nor is it to be expected any where in his large anatomy, becaufe, in the preface it, he declares, he is not to give the hiftory of the ufes of the parts, nor have I obferved it in his *exercitationes*.

Cowper, in the explic. of Bidloo's *tab.* xv. and in both the editions of his Myotomy, fays, "When we fwallow our aliment, we fhut our "mouth," which is true; but he mentions nothing of the impoffibility of doing otherwife.

Monro's words are, "Effays *vol.* 1. *p.* 111.;" One can fcarce fwallow any with his mouth open, which might be tranflated into French, "C'eft "avec beaucoup de difficulté qu'on peut avaler "quand la bouche eft ouverte," which I fuppofe Mr Ferrein will allow.—The experiment of fwallowing when liquor is poured into the mouth from above, does not prove it not difficult to fwallow with the mouth open; and in this fituation, the weight of the liquor fupplies, in a great meafure, the office of the digaftric mufcles.

The challenge of the accuracy of quotations, puts me in mind to remark, that Mr Platner, whofe pen is faid (*p.* 427—578.) to have been employed about the ufe of one of the mufcles of the lower jaw, does no more than relate fome of the things which authors, and particularly Mr

Mr Winslow and I had said, declaring, *p.* 14. *Nec in has lites ire, nec eas componere ausim.* He will neither enter into, nor determine this disputed question.

If any of your readers think I have wrote more, and you have published more than the subject in dispute deserves, they will, I hope, be so good as to remember, that, on my part, self-defence is allowable, and the example of the Academy of sciences, who have inserted 92 pages in 4to, on this subject, into their memoirs, may plead your apology. I might perhaps have omitted all the things in which Mr Ferrein agrees with me; but I began to fancy they were of value enough to deserve to be repeated, because this ingenious gentleman generally introduces them with *j'ay eprouvé, reconnú, trouvé, montré*, or some such phrase of approbation.

## Answer to Professor *WALTHER.*

IN a dissertation *de Oscitatione*, published at Leipsick in 1738, the professor, *p.* 12. criticizes my fourth objection to the office, commonly ascribed to the digastric muscles, in your vol. I. art. xi. The character which a very good judge * gives of this gentleman's writings, obliges me to transcribe his own words, that others may know whether I do him justice.

After giving a translation of my objection, he says, "Itaque, ut luculenter adpareat, quomodo "ille scriptor, hac doctrinæ suæ parte, motus "componat, qui aliter a se distingui debent, hinc "ita arguendo rem declaramus: Caput recli-

 "nare

* Haller. Method. stud. Medic. p. 548. dictione paulum obscuriore usus est.

" nare eundem affectum habet ac si in vivo
" homine id extendatur. Hinc idem erit, atque
" maxillam inferiorem ab inferiore, præsertim in
" cadavere, ubi hæc per se stupida cernitur, dis-
" cedere jubere, et ita os aperire. Porro, caput
" reclinare, idem erit, ac digastrici originis lo-
" cum demittere, et, si mansores musculi maxil-
" lam in vivis fixerint, hanc capitis reclinatio-
" nem, contractione digastrici juvare; sed præ-
" ter hunc casum, eundem musculum debilitare,
" et pro maxilla deducenda inertem reddere:
" nihilo minus tamen eundem, tanquam si liga-
" mentum esset, longiorem facere, et ita inten-
" dere, simul, os hyoides non propria, naturali,
" ac facillima contractione musculi, sed plurium
" partium communi tensione ac motu, levare.
" Nam omni occasione, os ipsum quoque tolli-
" tur, etiamsi maxillæ committantur, et tantum
" caput reclinetur; sicut, quod inter pomum
" Adami et os hyoides spatium est, ac illo tem-
" pore admodum augetur, etiam in vivis hanc
" rem probavit. Eadem fallacia communis at-
" que proprii motus adsertio nititur, cum cl.
" Monro præsumit; toties os hyoides deorsum
" duci, quoties maxilla vivi hominis descendit.
" Verum, ubi caput erigitur, et binæ maxillæ
" committuntur, quin et deglutitione accidit,
" ut priores hyoidei musculi et pars stylo-pharyn-
" gæi os bicorne aliquantum tollant, aut alium
" membranarum nexum hoc os sequatur, ut, di-
" gito altero ad mediam basin, et altero ad cornu
" admoto, facile percipitur: sed, quando jam
" maxilla movetur, tum hoc fere levissimo mo-
" do os hyoides demittitur: imo, eo tempore,
" sterno-hyoideos aut sterno-thyroideos musculos

" non

"non contrahi, vel exinde judicare Monro potu-
"isset, quod hic maxillæ motus, et oris hiatus,
"motus proportionem, quo os hyoides descendit,
"creberrime superet; et maxime, quod, ore
"clauso, perinde ac aperto, scutiformis cartila-
"go, sub osse hyoideo, in alterutrum latus, pa-
"ri facilitate converti possit."

The objection here, I imagine, is, that the *os hyoides* rises when the head is extended, and descends when the head is bended, though the mouth is shut all the time; the rising of the *os hyoides* in the former case being owing to the stretching of the parts, without any contraction of the muscles which are connected to the *os hyoides*, as the descent of this bone in the flection of the head depends on the natural elasticity or restitution of the overstretched parts stretched beyond their tone.

If I had proposed no more to be done, than to observe what happened to the *os hyoides*, when the head was alternately bended and extended, the objection would have been reasonable; but, as I desired muscles to be drawn with hooks and threads, while the posture of the head remained the same, and appealed to the feeling of muscles hard, swelled, and tense, while the different actions were performed by a living person, this objection can have no force.

It is to be wished too, we had been here told, how a muscle becomes weaker and unfit to do its office, by stretching it, and making it tense, as seems to be affirmed in regard to the digastric muscle; for, on the contrary, it is a doctrine generally received, that muscles exert most force when they are tense.

The particular objection to the sterno hyoidei, and

and ſterno-thyroidei muſcles acting in opening the mouth, to wit, that very frequently in this action, the lower jaw moves proportionally more, and the mouth is wider opened than the *os hyoides* deſcends, will not be inſiſted on by any who conſider, 1. That, ſuppoſing theſe muſcles to be the only depreſſors of the jaw, the chin would move downwards much more than the ſpace which theſe muſcles contract, or than the *os hyoides* deſcends. 2. That muſcles placed between the *os hyoides* and chin, contribute to the opening of their mouth by the contraction, the quantity of which is not ſo great as the defect of the chin, occaſioned by this very contraction, is.

To illuſtrate theſe propoſitions, and ſome others mentioned in my reply to the two learned academicians, and to ſhow ſome other things not generally attended to, in conſidering the motions of the jaw, allow me to make uſe of a little figure, repreſenting theſe parts in their motions.

Let A, fig 3. be the axis of motion of the lower jaw.——B, C, D, the chin. AB then is the jaw in the reclined or extended ſtate of the head. AC is the jaw in the middle or erect ſituation. AD is the jaw fully depreſſed.—Let E be the *os hyoides*. EB a muſcle fixed to the *os hyoides* and chin. EC, ED, the ſame muſcle ſhortened as the jaw deſcends. ED being in the ſame plane with the jaw, and EB perpendicular to that plane.——Let EF be a muſcle extended between the ſternum and *os hyoides*.

Hence it appears, 1. That the depreſſors of the jaw being fixed at a greater diſtance from the center of motion, than moſt other muſcles are fixed to their reſpective bones, have a longer lever to act with.

2. Suppoſing

2. Supposing the *os hyoides* E unmoved, while BE acts, the space which EB contracts, is so much less than the chin descends as B*g*, the difference between BE and CE, is less than the space between B and C.

3. Less shortening of the muscle EB is required as the chin descends, to make the chin move equal spaces, for C*h* or *gi*, is less than B*g*, tho' the space between B and C and between C and D are equal.

4. The angle ABE, is larger than ACE, or any angle which could be between the muscle and the chin, moving from B to D; and, the farther EB recedes from being perpendicular to AD, the angle between the bone and muscle always becomes less; that is, the angle of insertion of this muscle is decreasing as it continues to act.

5. If ever the muscle and jaw are in the same plane, as AE and ED here coincide, the muscle may draw the bone back, but can no longer depress it; and, if the bone was brought lower down by any other power, this muscle would raise it, instead of drawing it down.

6. The muscle EF acting may be considered as fixed to the point E, *e*, *e*, of the jaw; and therefore this muscle shortens itself so much less than the chin moves, as the spaces between *e* and *e*, or *e* and E are less than the spaces between B and C or C and D.

7. If the *os hyoides* E moves downwards at the same time that the jaw descends, then the muscle EB requires to have a less quantity of contraction, than when E is fixed, to move the jaw thro' the same space; from C to *k*, is longer

longer than EC, and the angle of infertion does not decreafe fo much, the angle AC*k* would be larger than ACE.

8. If E moves only forwards while the jaw defcends, the mufcle EB requires to fhorten itfelf more than when E remains fixed. A line drawn from C to *l*, is fhorter than CE, but the angle of infertion does not decreafe fo much; AC*l* is larger than ACE, and EF acquires a longer lever.

9. When the *os hyoides* is moved forwards and downwards while the jaw defcends, the angle of infertion of EB decreafes lefs than when it is fixed, and EF acquires a longer lever; but the quantity of contraction of EB may be more or lefs, or the fame, according to the proportion of the advancement forwards and of the depreffion of E.

## XIV. *An Account of a Child born with the Urinary and Genital Organs preternaturally formed; by Mr* James Mowat *Surgeon at* Langholm.

IN November 1732, a child was brought forth, whofe *funis umbilicalis* was tied to the upper edge of a deep hole, at the place marked C, (fee tab. II. fig. 3. where all the parts are reprefented of the fize they now are of; and juft above the *offa pubis* this deep hole penetrated the peritoneum; but now a lump of fpongy flefh D rifes out of it. The edges of the hole were at firft and are now round. From the fpongy flefh of this hole arife two papillæ A, B, about the fize of the point of an ordinary probe: At which

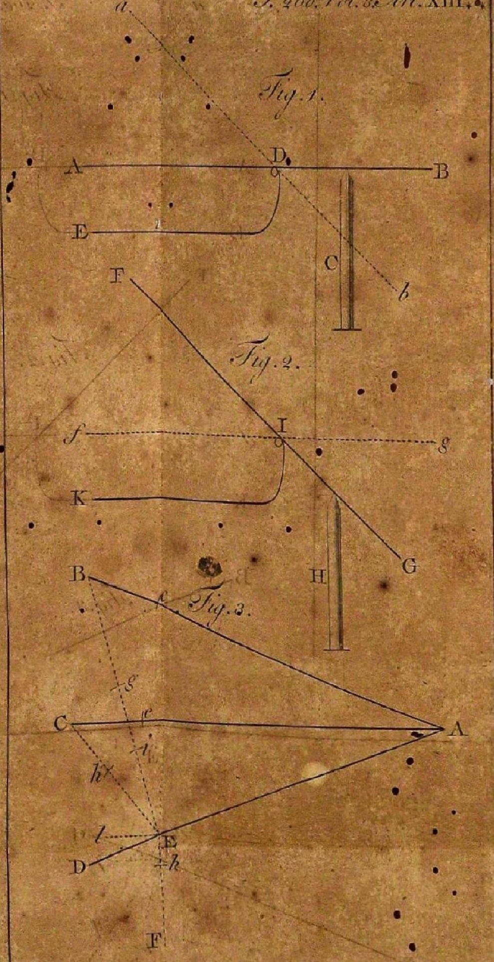
Fig. 1.
a
D
A
B
E
C
b
Fig. 2.
F
I
f
g
K
G
H
Fig. 3.
B
c
g
C
e
A
h
l
E
D
k
F

which the urine continually ouzes; but, when the child cries, the urine jets out as blood from the aperture of a ſmall artery. The papilla on the left ſide at A is now cloſed, but the other at B keeps open as at firſt, through which the urine continually ouzes, and, ſcalding all the parts, keeps them very raw.

The penis ariſes immediately at the under ſide of the hole, is now of the ſame bulk with the figure G, but was at firſt much leſs; it was and ſtill is imperforated, and flat upon the upper ſide next the glans, as if it had been ſlit longitudinally: It has two ſmall blue veins marked *o*, *o*, and a large prepuce inverted at H.

The ſcrotum and teſtes were and continue in a good condition; the ſcrotum KK is corrugated very cloſe to the end of two prominencies marked E, E; the raphe, I, appears in its middle with the wrinkles on each ſide; the teſtes, F, F, lie under the prominencies, they can be moved from the place they are ſeated in, either higher or lower as we pleaſe to put them.

The diſtance between the ſcrotum and anus is longer than it ſhould be; and the oſſa pubis are longer and flatter than in other children.

There is a large prominency, E, E, on each inguen, under which the teſtes lie.

The child is healthy, active, and ſtirring, and is very much affected with his misfortune; for he frequently cries upon viewing himſelf, and is very unwilling any other ſhould ſee theſe parts.

The mother is a very healthy woman, has brought

brought forth another child every way ſound and right in all its members. She tells that in May, before ſhe was brought to bed of this her firſt child, ſhe was ſtruck in the belly with a cow's horn; ſhe recovered the hurt in two or three days, but the fright remained longer with her, and did terrify her ſometimes in her ſleep.

I have twice or thrice thruſt down a ſmall ſilver probe at the right papilla B, about an inch or more, but cannot feel it in the perineum. I have alſo cloſed up the papilla two hours and three quarters with a ſmall tent and an aſtringent plaiſter over it, but can diſcover no ſwelling in the perineum, but rather in the belly. Upon withdrawing the tent, the urine ſquirted a great way; but by the uneaſineſs of the child, and the anxiety of the mother, I'm obliged to deſiſt at preſent from any further experiments.

## XV. *An Eſſay on the Diſeaſes of the Lacrymal Canals; by* ALEXANDER MONRO, *Profeſſor of Anatomy in the Univerſity of* Edinburgh.

THE many improvements that have been made of late in moſt chirurgical operations, as they ſhew how imperfect ſurgery was formerly, ſo they ſhould be an incitement for endeavouring to improve it ſtill further; which will be found no difficult taſk to any who carefully conſiders the natural ſtructure and ſituation of the parts that are affected in the ſeveral exter-

nal

Fig. 1.
Fig. 2.
Fig. 3.
A
B
C
D
E
F
G
H
I
K
L
R. Cooper sculp.

nal diseases; who examines the changes which these diseases do, or may make on the body; who from thence lays down reasonable intentions of cure, whereby the parts may be brought as near to a natural state as possible, or the functions of such as are disordered or destroyed may be supplied by art; and lastly, who diligently weighs the manner, immediate effects, and consequences of every step to be taken in executing these intentions. I shall endeavour to shew, in the following remarks on the *fistula lachrymalis*, how far the want of due attention to these necessary circumstances is capable of keeping us in ignorance, and leading us into error. I made choice of the *fistula lacrymalis* for an example, because it is a common enough disease that has been often seen and treated by surgeons, is wrote of in all the systems of surgery, and in most collections of observations, and is more particularly examined by the professed oculists; yet, in my opinion, is very little understood, and has very defective or faulty rules laid down for its cure.

I shall not trouble you with critical observations on the old distinction of anchylops and ægylops, or on the impropriety of reckoning all ægylopes or ulcers of the internal canthus of the eye to be *fistulæ lacrymales*, or on the characters of any ulcer necessary to constitute a fistula; but shall only inform you, that the disease I now treat of, is such an indisposition of the canals that convey the tears from the eye to the nose, as does not allow the tears to pass as they ought: If you think the name of *fistula lacrymalis* does not agree to this description,

you'll

you'll do a favour in changing it for a more proper one, or in assigning names to the different cases I shall suppose.

The lacrymal canals, whose diseases are the subject of this essay, having been described by several anatomists, but, none of them having these diseases in view, their words or pictures will not probably give your readers an idea of these parts suited to my purpose; wherefore I shall give a short description of them, illustrated by the figures that are sent with this paper, before I enter upon any account of their diseases. (See tab. III.)

The two lachrymal points, A, B, (Fig. 1.) situated each on a little prominence near the internal extremity of the edge of each palpebra, take in the tears to be conveyed by two little ducts of about four tenths of an inch long, which are continued from the points inwards and somewhat downwards, (the superior being the longest and most oblique), till they open into the lacrymal sac, D. Between the points and the angle where the palpebræ join, the *caruncula lacrymalis*, C, is placed. The lacrymal sac, D, lies upon the groove formed partly in the nasal process of the maxillary bone, and partly in the anterior half of the *os unguis*, to which its back part adheres slightly; but this membranous bag is connected firmly to the ridge which is raised on the *os unguis*, at the posterior part of the groove, serving at this place as a proper distinguishing boundary between the orbit and exterior parts; so that the lacrymal half of the *os unguis* is without the orbit, while its posterior half constitutes a share of the bony sides of that cavity.

Such

Such another firm connexion of the lacrymal sac to the bones is also to be observed at the fore part of the groove, where a small suture joins the *os unguis* to the nasal process of the maxillary bone.

The lacrymal groove of the *os unguis*, D, is about two tenths of an inch broad in its middle widest part, and is about half an inch long from the top, till it is covered by the maxillary bone, and a compleat bony canal is formed for inclosing the whole lacrymal duct; which, after a short progress, opens into the nose immediately below the middle of the superior edge of the lower *os spongiosum*, where its extremity becomes smaller than any other part of it.

When we view the side of the nose after the bones have been divided by a perpendicular section, we see the *ossa spongiosa*, K, L, fig. 2. situated near horizontally, depending by their superior edge from the other bones, and removing farther from them as they descend. The anterior extremity of the superior *os spongiosum* K, is fixed to the other bones very near where the upper part of the *os unguis* is joined to the frontal bone; and the superior edge of the inferior, L, is very little below where the great lacrymal duct begins.

This short description will, I hope, assist your readers to understand the several morbid cases I am now to consider.

If, after any erosion of the eye-lids, the lacrymal points, or the small pipes going from them to the lacrymal sac, are intirely blocked up by their sides growing together, which may be known by the constant weeping of the affected

 eye,

eye, after a diſeaſe capable of producing ſuch an eroſion, without any tumor, but on the contrary with a depreſſion of the teguments covering the lacrymal ſac, and by the points being ſo obliterated, that one of Anel's ſmall probes cannot be puſhed by them into the ſac; in ſuch a caſe, I ſay, the patient muſt all his life bear the deformity and uneaſineſs of a weeping eye, or ſome ſuch operation as the following muſt be attempted: Let the lacrymal ſac be opened in a ſlow cautious diſſecting manner; after which puſh a ſmall round curve needle with a waxed thread from one of the prominencies of the palpebræ, where the lacrymal point naturally is, into the ſuperior part of the ſac; draw out the needle at the aperture lately made, and leave the thread by way of a ſeton; do the ſame at the part where the other *punctum lacrymale* was. Soon after the ſmall inflammation theſe threads may raiſe is over, the briny tears trickling along them will make the paſſages callous and fit for ſupplying the office of the natural ducts, when the threads are to be taken out; and the aperture in the ſac, which has been kept open by doſſils, and refreſhing ſometimes with the lunar cauſtic, will very readily ſhut up as ſoon as this manner of dreſſing is forbore, and that it is only covered with a pledgit. The ſucceſs with which an artificial paſſage, formed this way into the mouth, has ſupplied the ſalivary duct (See *art.* XIII. *vol.* 2.) may make us judge, that the method juſt now propoſed might alſo be ſucceſsful.

When the fibres of the lacrymal ſac are too weak, or the large duct is obſtructed by ſome concreted

concreted liquors, the sac is gradually stretched by the tears which regurgitate frequently at the *puncta lacrymalia*. Some call this disease a dropsy, others would have named it a hernia of the lacrymal sac. We know it by the tumor of the sac without hardness, discolouring, or pain, which disappears as soon as we press out the tears at the *puncta lacrymalia*. While this disease is recent, these tears are pure, afterwards some pus appears with them, because of the excoriation which the sac suffers. Lest there should be any hazard of mistaking a tumor or small abscess in the teguments, which cover the lacrymal sac, for the disease of the sac just now described, as I have seen done, allow me to mention, that such tumor is easily distinguished from the hernia or dropsy, by its not diminishing or not discharging a large quantity of tears or pus at the *puncta lacrymalia* upon pressure.

The method of cure in the dropsy, is to pass one of Anel's probes from the *puncta lacrymalia* into the nose, to remove any grumous matter that may be lodged in the lacrymal canals, and then to inject, by the lacrymal points, mild, detergent, and gently astringent liquors; such as mel-rose diluted in lime-water, to which a little brandy may afterwards be added, or any chalybeat water, or a little weak wine, &c. which injection is to be repeated twice or thrice a-day; and in the intervals the cure is assisted by external compression, made with compress and bandage, or the proper compressing machine, and by corroborants. We read of several cures performed in this manner by Mr

Anel and Mr Heister; I have also had success with it.

It will be necessary here to observe, that the small duct, going from the superior lacrymal point, A, fig. 1. being more oblique than the inferior, it will be more proper for passing the probe by; and, because the passage from that point into the nose is not straight, the probe must be bended into a small arch of a large circle. The superior eye-lid being then raised, and its edge turned a little outwards with the fingers of one hand, the surgeon, resting the other hand on the patient's cheek near the exterior canthus of the eye, introduces the probe, with its convexity upwards, into the punctum, and raising his hand gradually as he pushes the probe forward, he brings it almost perpendicular to the lacrymal sac, by the time that the probe reaches the lower part of that sac; then he turns the probe softly, till he brings its concavity towards the nose; and pushes it downwards thro' the great duct into the nose. After which he moves it up and down, and to different sides, to break any concreted matter lodged in it.

The inferior *punctum lacrymale*, B, will be fitter for introducing the pipe of the small syringe into, and for making the injections by, because the inferior eye-lid has not near so much motion as the superior, and is more easily held with its edge turned a little outwards, at the same time that the surgeon has a better rest on the patient's cheek for the hand that holds the syringe, than he can have in making the injection by the superior point.

A

A propofal has been lately made to introduce a fmall pipe at the lower part of the lacrymal duct, into the fac, by which medicines may be conveyed for curing feveral difeafes of the lacrymal canals; but I don't know whether cures have been made with this pipe.

If thefe methods of injecting and compreffion are not fuccefsful, and the internal part of the lacrymal fac is become fpongy and ulcerated, which we judge to be the cafe, by the quantity of pus expreffed with the tears, the fac muft be opened by an incifion. When the fac is much diftended, and the matter in it is thick, it is as eafily opened as any common abfcefs; but, when the fwelling is little, and the matter is thin, I have obferved, that, notwithftanding the fkin was kept as tenfe as I could betwixt my thumb and two fingers, and an affiftant endeavoured all he could by preffure on the *puncta lacrymalia*, to prevent the tears and pus efcaping by thefe orifices; I fay, notwithftanding thefe precautions, I obferved that the preffure of my knife fqueezed out the liquors contained in the fac, and made it collapfe fo much, that it could not be opened without a manifeft rifk of cutting the pofterior fide of the fac, and fo laying the bone bare, which evidently appears neceffary to be fhunned in the cafe I now fpeak of. When the fac is full of fpongy flefh, or has contracted after an unfuccefsful attempt of the operation, it is fcarce poffible to know when the fac is opened, or how much of it is opened, by an incifion, without having fome directory for the knife. To avoid thefe inconveniencies, in fuch cafes, I introduced a fmall probe at one of the *puncta*

*lacrymalia*, and caused an assistant to raise up the sac with it, while, with a gently-crooked sharp-pointed bistory, I cut the stretched teguments in the common way, till I felt or perceived the naked probe; when, laying aside the bistory, and taking a pair of crooked scissars, I introduced the probe-pointed blade into the sac, and cut it first upwards, and then downwards, till its whole length was opened.

In making this opening, the tendon of the orbicular muscle of the eye-lid must be cut through; but it is of no consequence, for the firm cicatrice afterwards ties that muscle to the bones here sufficiently, to prevent any inconvenience. We are however to take particular care not to cut so near to the joining of the palpebræ, as to be in any hazard of dividing them, which might occasion a considerable deformity; and it will be more convenient to save the angular artery and vein, than to wound them; because, if they are wounded, the blood which they pour out hinders the operator to see so distinctly what he is doing.

After the sac is fully opened, we can observe in what condition it is, and are at liberty to free the nasal duct of any thickened matter that happens to be in it. Small dossils, armed with some proper medicines, either of the detergent, drying, or strengthening kind (according to the morbid state of the sac) are laid into the sac, but without being made very hard, or being stuffed strongly in, lest unnecessary pain and inflammation should be occasioned. The lips are then covered with a small pledgit, and this is kept on by a semilunar snip of adhesive plaster;

ſter; and, if the dreſſings do not keep well enough in the ſac, compreſſes, and the compreſſing inſtrument, ſhould be put upon them. While the diſeaſe is a curing by proper medicines, the lips are kept freſh with the lunar cauſtic. When once the ſac is made ſound, the orifice in the teguments cloſes very ſoon after the uſe of the doſſils is forbore, if the patient is in any thing of a tolerable habit of body. I have practiſed this method with ſucceſs.

When the large *lacrymal duct* is excoriated, or has fungous fleſh riſing from it, which will be known by the acute pain, or great inſenſibility, and by the difficulty of paſſing a probe through it after the ſac is opened, and by a view of its ſuperior part, there is a neceſſity of dropping or injecting proper medicines into it, and of keeping its ſides from becoming contiguous, by introducing ſome convenient ſubſtance into it. When medicines are made to paſs through it, the patient muſt be deſired to hold his head forwards, that the liquors may run out at his noſe, inſtead of falling back into his fauces. In my opinion a ſmall tent of lint, ſecured with a thread, and armed with medicines, is preferable to a ſmall wax bougie, or any thing that is oily, becauſe theſe keep the parts raw much longer; and do not imbibe ſuitable medicines. As ſoon as the duct is brought to a right condition, the diſeaſe is the ſame as in the preceeding ſuppoſition.

Let

Let us now ſuppoſe, that the fungous fleſh riſing from the ſides of the duct, has united and blocked up the paſſage entirely, which can only be diſcovered after the ſac is open, by the impoſſibility of introducing a probe, or making liquors paſs through it, into the noſe, while it feels ſoft and yielding, without that ſort of firm reſiſtance which a bone covered with a membrane makes, and which cannot be deſcribed well in words, but what all ſurgeons of any practice know. In the caſe as I have now put it, I think there is ſtill no neceſſity of hurting the bones, in order to make a paſſage for the tears. What I would propoſe is, to puſh a ſmall ſhoemaker's awl, or ſome ſuch inſtrument, through the middle of the fungus into the noſe, and then to keep this artificial paſſage open, and to render it callous by a tent or ſeton. In making the perforation, the inſtrument muſt be held with its concavity towards the noſe, and it muſt be thruſt through ſlowly, and with no great force, the ſurgeon changing its direction a little when ever he touches the bone with its point; when the drops of blood coming out of the noſe, ſhew the inſtrument to have perforated far enough, it is drawn back, and the tent or ſeton muſt be immediately paſſed in the ſame way. The ſeton is preferable in my opinion, but requires the probe by which it is introduced to be of very flexible ſilver, and previouſly brought to a particular form. You ſee, *tab.* III. *fig.* 3. a probe about three inches long, bended into a ſemicircle, only with near half an inch toward the point pretty ſtraight; this I have made to paſs from the lacrymal ſac

into

into the nose, and brought it out at the nostrils of several dead bodies, without using force, or changing its form. The small cord that is brought through in the eye of this probe, is to remain for some days, without shifting the part engaged in the duct, till it becomes loose by the suppuration which happens round it; then it may be drawn a little, having besmeared the part that is to be next introduced with some gentle suppurative balsam. In a very little time the suppuration will waste as much as is sufficient; and then the detergents and deficcatives will succeed in bringing the canal near to the natural state. I need scarce mention the tying of the two ends of the cord after each dressing, to keep them from hanging over the face, whereby they would be in hazard of being unwarily pulled, or the necessity of dressing the sac all the while, as in the former supposition; or taking out the cord and curing all up, after the duct and sac are sound, they are so obvious.

If the duct has been blocked up in a child, and no cure is attempted till the person comes of age, the duct may be so obliterated or small, that the method now proposed cannot be executed, and an artificial passage must necessarily be made through the bone: But as this case can never be certainly discovered till the sac is opened, we ought still to proceed in the cautious way I formerly mentioned for this part of the operation. The place of the *os unguis* where this perforation ought to be made, will easily be determined from the description I gave of the

the parts, and from the advantage of the canal for the tears, being at the most depending part of the sac. It must not however be attempted to be made where the natural duct was, for, in piercing the bones in that place, and with that direction, the instrument will more readily pierce into the large *sinus maxillaris*, than into the nose. If the perforation is attempted to be made too far forwards, the instrument must be forced through the maxillary bone, which cannot be pierced but with great difficulty. I imagine any one may easily understand the place and direction for making the perforation right, by observing how the two pins are placed in *fig.* 1. and how they come through the *ossa spongiosa* in *fig.* 2. at M and N; for the pin E, in *fig.* 1. being thrust perpendicularly through the *os unguis*, about the middle of the lacrymal sac, pierces the anterior extremity of the upper *os spongiosum* at M, in *fig.* 2 and the pin, F, in *fig.* 1. thrust very obliquely through the *os unguis*, at the lowest part of the sac, pierces the *os spongiosum inferius* at N, in *fig.* 2. The pin, F, placed so far back as to be contiguous to the ridge dividing the *os unguis* with the perpendicular direction of the other pin, E, would answer all intentions without any risk.

The instruments, with which this perforation has hitherto been ordered to be made, appear to me very faulty. One general fault to all of them is, their destroying more of the *os unguis* than is necessary or safe; for, where-ever the orbitar part of it is diseased, there is great danger of an inflammation and suppuration be-

ing-

ing brought on the muſcles and fat within the orbit, which may be attended with a train of troubleſome dangerous ſymptoms, that art can do little to relieve, becauſe of the quantity of fat, in which pus diffuſes itſelf eaſily, and cannot be reſtrained by medicines or compreſſion in ſuch a cavity as the orbit, and in the neighbourhood of ſuch a ſenſible moveable neceſſary organ as the eye. The actual cautery gives great pain, burns the neighbouring parts, raiſes inflammation, and leaves a carious piece of bone to exfoliate, which retards the cure much. The directory, or blunt ſtiler, when puſhed through the bone, fractures it far and near, and often ruſhes into the noſe ſo far as to break the *ſeptum narium*. The olive-ſhaped blunt perforative, or the tapering ſtrong forceps, make large fractures in the bone, beſides opening a paſſage large enough to let the point of one's finger paſs, where one no larger than a crow-quill is required. In place of all theſe then, I would propoſe always to make uſe of a drill, ſmall perforative of a trepan, gimblet, trocar, or any ſuch ſmall inſtrument that can perforate with little force and no fracture. The trocar is the inſtrument I have made this perforation with ſeveral years paſt, in the following manner. Chuſing a trocar the ſtilet of which is of the ſize I wiſh to have the perforation, and is as much longer than its cannula as I think neceſſary for perforating into the noſe, I put a probe into the cannula, to ſearch for the loweſt part of the ridge of the *os unguis*, at the fore-part of which I keep the probe till the cannula is

preſſed

preſſed ſoftly down to that part of the bone. When the probe is taken out, the cannula is put in the proper direction, and the ſtilet of the trocar is then put through the cannula, to be worked with half-turns of the hand till it perforates into the noſe, in doing which its own weight is ſufficient, ſcarce is any preſſure of the hand required.

So ſoon as the perforation is made, withdraw the ſtilet, to paſs a tent ſecured with a thread, or a piece of wire-drawn lead, through the cannula into the noſe; then, taking away the cannula, accommodate the tent or lead to the reſt of the wound; or the cannula may be taken away with the ſtilet after the perforation, and the tent or lead, or a little pipe, may be put into the hole, having the head of the tent broad, or the lead ſplit a little at its outer end, that each part may be bended down to make a head, and the pipe ſhould have little ſhoulders to prevent it from falling into the noſe: Then dreſs the lacrymal ſac in the ſame way as was propoſed above, when we ſuppoſed the ſac to be laid open. Allow theſe dreſſings to remain till the ſuppuration comes on, when they are to be renewed. Whenever the inflammation is gone, by drying medicines injected at the new orifice, or conveyed into it by the tent, endeavour to harden the membrane with which the thin edges of the perforated bone ſoon cover. I uſed mel-roſe and a little brandy, increaſing gradually the proportion of this laſt medicine. Whenever the tent can be made to paſs this hole, without giving pain, leave off the uſe of the tent or pipe, and cure up the external

external orifice, as ſoon as it will go together, which is very ſoon, if its lips have been gently touched from time to time with the lunar cauſtic." In this way I have cured thoſe who had this diſeaſe from their infancy, without one bit of bone exfoliating, or the leaſt weeping in the eye afterwards, or other inconvenience, not ſo much as an obſervable ſcar.

I am informed that a gold pipe has ſometimes been left in the new paſſage; the cut fore-part of the ſac having united, while it remained there.

Let us now ſuppoſe, that the ſharp matter in the lacrymal ſac has deſtroyed its membrane, and rendered the *os unguis*, on which it lies, carious, or that the caries having begun in the bone, the ichor of it has eroded the membrane. In this caſe, if there is a large paſſage eroded alſo thro' the *membrana narium*, while the teguments are whole, it may be long before the lacrymal canals can be diſcovered to be affected; and the diſeaſe will be treated as an ozæna. But, if there is no ſuch paſſage into the noſe, the malady may be known by the brown-coloured ſtinking ichor diluted with tears, which may be ſqueezed out at the lacrymal points, upon preſſing the lacrymal ſac.

The method of cure here will be to open the lacrymal ſac, as in the former caſes mentioned, to ſeparate as much of the bone as is carious, to make a perforation with the point of a lancet or biſtory, through the *membrana narium*, and then, to complete the cure, as directed in the

the preceeding ſuppoſition of the bone being artificially perforated.

The ſeparation of the carious bone is ordered to be haſtened by the application of the actual cautery, tinctures of myrrh and aloes, and of euphorbium: But, in my opinion, the breaking away with a pair of forceps all that is carious will be much more ſpeedy, and is not attended with ſuch inconveniencies as the other methods are.

You muſt have obſerved, that I have hitherto ſuppoſed the diſeaſes of the lacrymal canals to be attended with no opening of the teguments made by eroſion, nor with any maladies of the neighbouring parts; and, I believe, you will ſee there is no neceſſity of inſiſting at any length upon them. For, when there is an opening in the teguments, near the internal canthus of the eye, we can eaſily diſcover whether the lacrymal canals are affected, by preſſing pus out of the *puncta lacrymalia*, before the ulcer is cleaned; and, after the pus is wiped away, the tears will run out at the external orifice; which alſo gives a better opportunity of introducing inſtruments to diſcover the ſtate of the diſeaſed parts.

In the cure there is nothing different from what has been formerly directed, unleſs that the opening into the ſac is more eaſily made, where the external orifice is large enough to allow the neceſſary inſtruments to be introduced; and, when it is too ſmall for this purpoſe, we muſt enlarge it, by putting into it tents of ſponge made firm and hard, by being ſoaked in ſome melted

melted plaster, and then kept pressed under a weight, or in a press, till the plaster hardens; or this sponge-tent may be prepared, by simply wetting the sponge in water, or a diluted mucilage or glew, and then rolling pack-thread firmly round it, and hanging it up thus till it dry.

There is such a great variety of diseases which may accompany these maladies of the lacrymal canals, whether as causes, consequences, or accidental attendants, that it would be to engage in almost a system of physic and surgery to give a detail of them; and therefore I shall pass them without any further examination.

XVI. *A Tumor of the Nose unsuccessfully extirpated; by——*

THE author of art. XXII. in your first volume, having had the benefit of your promise to conceal the names of those who send you unsuccessful cases, I claim the performance of the same promise in the publication of this paper, if you think it deserves a place in your collection.

A child was born with a small moveable tumor on its nose, which increased as the child grew, otherwise the child was healthy and strong, having only had a short fever or two, and passed some worms, before five years of age, when my advice was first asked concerning that tumor, which now was so large as to cover all the nose, except the nostrils, and was so pro-

minent to each ſide, that the eyes were in part covered with it. Towards the baſe, it was ſo ſoft, that, by preſſing a finger on each ſide, they felt each other, but at the moſt prominent part there were ſeveral hard round knots. When I ſaw it, the child complained of no pain, tho' I was informed that ſharp lancinating pains ſometimes ſtruck through the tumor. I held the noſtrils, while the child forced its breath that way, but ſaw not the tumor riſe any. I ſearched into the noſtrils with a probe, but could neither feel any excreſcence, nor puſh the tumor or teguments outwards. The *os frontis* was firm, and united in the middle. From all which I concluded the bones of the noſe to be compleat, and therefore was of opinion the tumor (which would make the patient very miſerable by increaſing, and would bring death at laſt) might be ſafely extirpated. Being, however, taught by *Job à Meekren* *, and ſome others, how deceitful excreſcences of the head, brought to the world with a child, might poſſibly be, I would not undertake the cure, till another ſurgeon, of more experience and longer ſtanding, in my neighbourghood, who juſtly has a conſiderable character, ſhould examine the tumor, and aſſiſt me in whatever was determined to be done. That gentleman joining in opinion with me, I undertook the extirpation with his aſſiſtance. When I had diſſected about half the baſe of the tumor off, I obſerved the bones of the noſe to be incompleat; and that the membrane of the noſe, part of which I had laid bare,

* Obſerv. cap. 7.

bare, was moved outwards in expiration, and inwards in inspiration. Not being certain how far upwards the bones might be wanting, I dissected all the tumor off at the lower part, but left a little of its base above. Having stopped the blooding, I dressed the wound in the common way.

When the tumor was examined, it appeared all of a substance little firmer than the common fat under the skin, except where the knots were, which were of a schirrous hardness.

The child passed the first night pretty easily. Next morning the pulse was a little quick, attended with a thirst, and a sickness at the stomach, which had made the patient vomit once. An emollient clyster being injected, and emulsion given for drink, these symptoms abated. Towards the evening the dressings seemed moister than they commonly are so soon after a wound.

In the morning of the second day after the operation, the dressings, child's hair, and headcloaths, and the pillow under its head, were all wet with a watery liquor, which had a particular smell that I never felt in any wound before, and do not know how to describe. The dressings being taken off, we saw that this liquor ouzed fast from the bared membrane of the nose, though we could not perceive the orifice by which it escaped. We applied from time to time, *bol. armen. pulv. helvet.* chalk, sugar of lead, white vitriol, burnt allum, blue vitriol, quick-lime, brandy, alcohol. oil of turpentine, spirit of nitre dulcified, plain spirit of nitre,

oil of vitriol, lunar cauſtic, the actual cautery. In ſhort, we applied every thing we could think of that had any chance for ſtopping this oozing of lymph, but without ſucceſs. On the ſixth day, our patient vomited a long round worm; in ſome time after fell into convulſions, and in an hour more died.

XVII. *An Account of a* Procidentia Uteri; *by* ALEX. MONRO, *Profeſſor of Anatomy in the Univerſity of* Edinburgh.

——Scobie being ſeized with a fever, which continued ſome days, in the month of Auguſt 1728, when ſhe was not full three years old, had a conſiderable diſcharge of blood by the vagina for three days; after which ſhe ſeemed to be in perfect good health about twenty days, then complained of pains in her belly, loins, and thighs, and had ſuch another evacuation. The quantity of blood voided was judged by her mother to be as large as what ſhe herſelf commonly had in her menſes. The child ſuffered regularly ſuch returns every three weeks, or, at furtheſt, within the month, attended with the ſame ſymptoms, without any conſiderable loſs of ſtrength, or decay of her body, till the month of May 1729. But, during the third monthly evacuation, which was at the end of September 1728, her mother obſerved a ſmall ſwelling riſing out from the orifice of the vagina, which diſappeared as ſoon as the hæmorrhage ceaſed. This tumor, however, came out larger at each period thereafter; but, upon the child's

child's being kept in bed three or four days, and the flux of blood ſtopping, always diſappeared, till May that it came out of a conſiderable bulk, and did not return as uſual. From this time there were no more periodical evacuations of blood; but, inſtead of theſe, there was a perpetual dropping of a white mucus from a hole in the lower part of the tumor; which mucus was ſometimes in ſo large quantities, that, if a ſwath had been applied ſome hours about it, to prevent that liquor from coming away in drops, as frequently was done, whenever the ſwath was taken off, the mucus was thrown out ſo abundantly, and with ſuch force, as made thoſe preſent to imagine it was urine which the child paſſed.

About the end of July, the parents having brought the child to the hall of the college of phyſicians, where Dr John Riddle and Dr William Porterfield were then attending to give advice to the poor; theſe two gentlemen having viewed the child, deſired the parents to carry her to me.

Being informed of the preceeding hiſtory by the child's mother, I examined the parts, and found a tumor, G, (ſee tab. IV.) hanging out at the vagina, as big as a hand-ball, the neck of which, F, was about an inch diameter. At the loweſt part, H, the tumor was largeſt, and of a faint leadiſh colour: Behind the moſt prominent part of it, I diſcovered a hole of $\frac{1}{4}$ inch diameter, by which I introduced a probe, I, ſome inches; and then the probe was reſiſted, and the child complained of pain. From this hole, there

there was a conftant ftillicidium of mucus. Round this orifice the tumor felt hard and firm; but a little higher, where it was largeft, it was fofter, feeming to be compofed of a cellular fubftance; at this place fcales had frequently formed and fallen off. The neck, F, of the tumor was very fmooth, of a fhining red colour, and very folid and hard; I introduced a probe betwixt this neck and the fides of the vagina, two inches upwards, and turned it all round the circumference of the neck. The clitoris, D, nymphæ, B, B, and orifice of the urethra, E, were natural enough, only the neck of the tumor preffing on the urethra occafioned fome difficulty in the excretion of urine; and the urine being diffufed over the *labia magna*, A, A, and other neighbouring parts, by ftriking againft the large bafe of the tumor, had fomewhat excoriated thefe parts.

The child could fcarce fit, and ftradled when fhe walked, but lying a-bed fhe was very eafy. Her complexion was pale, and her body fmall, otherwife fhe was healthy. Having confulted with the two gentlemen who had fent her to me, and feveral other phyficians having feen her, the difeafe was unanimoufly judged to be a *procidentia uteri*. Wherefore I attempted to reduce it; but the tumor was fo large and firm, I could not accomplifh it. Fomentations and cataplafms, firft of the emollient and difcutient kind, were applied, afterwards they were formed entirely of the attenuants, and laftly aftringents were tried. In the mean time the child underwent the general evacuations as much as her ftrength could bear, without the tumor's yielding

ing in the leaſt, but, on the contrary, daily increaſing; at laſt ſhe began to turn hectic, and the tumor to be diſpoſed to gangrene on its outer ſurface, which were in vain endeavoured to be prevented by diet and antiſeptic medicines. I ſeveral times conſidered of the amputation; but, being ſenſible of the body of the uterus being to be cut through, and frighted by the ill ſucceſs Ruyſch and ſome others had in this operation, I had not courage enough to undertake it. The child in the mean time turned weaker, the tumor gangrened in its external ſurface, and, by the gangrened parts falling off, it was reduced to near half its former bulk. Ten days after which, (7th November), ſhe died.

On opening the abdomen next day, the bladder K, was full of urine, the left ureter, M, was in a natural ſtate, but the right one, N, was diſtended, by urine, to four times its natural diameter, and the kidney from which it came was larger, ſofter, and paler than the other, but without any appearance of the folliculi or veſicles ſometimes found in morbid kidneys. The urine had certainly been retained in the bladder by the neck of the preternatural procidentia preſſing on the urethra, and the diſtenſion of the right ureter was owing to a ſteatomatous body, U, ſome more than an inch long, and ſeven tenths of an inch broad, which lay behind the ovarium and *ligamentum latum*, and reached to the cervix of the bladder, to which it firmly adhered, and through its exterior extremity the ureter paſſed.

There

There was fcarce any thing of the uterus to be feen, till the bladder was reclined over to one fide, when a fmall part of its fundus, O, appeared.

The *tubæ fallopianæ*, Q, Q, were near perpendicular to the uterus and the ovaria, T, T, were fituated contiguous to them.

Having made thefe remarks, and carefully obferved the fituation in which the feveral parts were, I diffected off the peritonæum and its cellular membrane from the bones and mufcles compofing the fides of the pelvis, and brought away all the parts contained in that cavity with the right kidney and ureter; and then, that a view of the whole might be had in one figure, I diffected the left fide of the bladder away from the peritonæum, and reclined it over to the right; after which, having, with a needle, paffed threads through the fkin where the *mons veneris* and external or great *labia pudendorum* are, I gently ftretched the fkin of thefe parts, and fecured it in that pofture by help of the threads which were tied to a probe, and two pins which I had made faft to the table; in which pofture Mr Cowper delineated it, having his view obliquely from the left fide and from above. The figure he drew, and afterwards graved, will, I believe, better explain the fituation and connection of all the parts, than any defcription, and is the only one I know that gives a diftinct idea of this difeafe my patient laboured under, a true genuine *procidentia uteri* covered with the vagina, and without any inverfion of the womb.

A. A.

A. A The two great *labia pudendorum*.
B. B. The nymphæ.
C. *Præputium clitoridis*.
D. *Glans clitoridis*.
E. The orifice of the urethra.
F. The neck of the procidentia as it came out at the vagina.
G. The left side of the tumor, which was much diminished by the falling off of the gangrenous parts.
H. The right side which had no parts cast off.
I. A probe put into the uterus by its internal orifice.
K. The bladder distended with urine, and reclined over to the right side.
L. L. The jagged edges of the peritonæum both on the left side of the bladder, and of the pelvis where it was cut, to remove the bladder to a side.
M. The left ureter of the natural size.
N. The right ureter greatly enlarged with urine.
O. The *fundus uteri*.
P. P. The *ligamenta lata*.
Q. Q. The *tubæ Fallopianæ*.
R. The fimbriæ of the right tuba with its orifice in view.
S. The left *morsus diaboli* seen on the side averse to the orifice.
T. T. The ovaria.
U. The extremity of the steatom appearing from under the right ovarium.
W. The thick *tunica cellulosa* at the side and back-part of the pelvis.
X. The *intestinum rectum*.

Y. The

Y. The probe to which the thread supporting the *mons veneris* was tied.

Z. Z. The pins to which the threads ſtretching the great labia were faſtened.

After the figure was drawn, I endeavoured to diſcover by diſſection, how far the inverted vagina or uterus had each been increaſed in their bulk to form ſuch a large tumor; but they were ſo intimately united, that I could not diſtinguiſh the ſubſtance of the one from that of the other; and therefore could not determine their proportional thickneſs.

## XVIII. *The Deſcription of a Peſſary, invented by* THOMAS SIMSON, *M. D. Profeſſor of Medicine in the Univerſity of St* Andrew's.

THERE is no calamity that afflicts the fair ſex more than the *precidentia uteri*, when they labour under it; for it is accompanied with perpetual uneaſineſs through their whole body: It gives the greateſt hindrance in following out the common affairs of life, and frequently ends in ulcerous and cancrous tumors.

The cauſes of this diſeaſe are many; but, which ever takes place, there is no hope of a cure, unleſs the part is kept in its natural ſituation; for its weight, when it hangs unſupported, does more harm than there can be ſervice done by any medicines applied for ſtrengthening and bracing the relaxed fibres; and therefore the firſt ſtep towards a cure of *precidentia*

*procidentia* has always been to reduce the uterus to its natural ſituation, and to keep it there by means of the inſtruments named *peſſaries*.

All the peſſaries which I have ſeen deſcribed by authors, or uſed by practiſers in medicine, ſeem to me defective and inconvenient; for they anſwer the deſign by their bulk only, and muſt be thruſt violently into the vagina, which not only occaſions a great deal of uneaſineſs and pain to the patient, but a ſkilful hand is always required to manage them; which many women are unwilling to allow. To evite the many diſadvantages attending the uſe of the common peſſaries, I contrived the one delivered with this paper, which, I can aſſure you, after ſufficient experience of it in ſuch caſes, has exactly anſwered all the intentions without any inconvenience.

Fig. 1. of tab. V. repreſents the exterior ſide of one of the plates of tin, of which the inſtrument conſiſts.

A, is the body of it, the ſides of which, B, B, C, have holes made in them for ſewing a piece of oiled leather on it.

D, a narrow neck in the form of half a hinge going out from the large plate.

E, a round plate full of holes, for ſewing upon it a hemiſphere of cork boiled in oil.

Fig. 2. ſhews the form of the other plate with its anterior ſide expoſed to view, and with the leather and hemiſphere of cork ſewed to it.

A, the plate at the ſides of which, B, B, C, the edges appear turned in, with the threads

which ſecure the leather on its outſide croſſing over them.

D, a ſpring of japanned ſteel, which is faſtened to the plate at E, but ſtands out from it at the other extremity.

F, the convex ſide of a hemiſphere of cork, ſewed to ſuch a plate as E repreſents in Fig. 1.

Fig. 3. is the figure of the inſtrument mounted.

A, B, the two large plates.

C, the ſpring keeping them at a diſtance.

D, one of the ſmall round plates with the threads, by which the hemiſphere of cork is faſtened to it.

E, E, the two hemiſpheres of cork.

F, waxed threads made to croſs from one hemiſphere of cork to the other, and left of ſuch a length as to allow the ſpring full play, in ſeparating the plates and corks.

G, G, two ſkains of waxed thread paſſed through the ends of each plate, and ſecured from being drawn out by the knots at their extremities.

Having ſeen D, the half of a hinge in fig. 1. one can eaſily imagine what could not be ſeen in this picture, viz. ſuch another applied to the ſide of it, when theſe necks of the two plates are made to croſs, and that a ſmall axis being put through them, they move eaſily.

When this inſtrument is to be introduced, the two plates are preſſed cloſe together, and the ſphere of cork is put as high up into the vagina as is convenient, taking care to have the flat ſides of the plates towards the right and

and left of the vagina. Whenever the fingers preſſing the plates are removed, the ſpring puſhes the plates and hemiſpheres away from each other, to preſs only on the ſides of the vagina, without any danger of ſtraitening the urethra or rectum. The croſs threads now extended between the hemiſpheres, hinder the uterus or plicæ of the vagina to fall down between the corks, ſo as to be in danger of being bruiſed, when the ſides of the inſtrument are again preſſed together, in order to take it out; and at the ſame time liquors will paſs freely. The corks and plates covered with leather, when preſſed againſt the vagina by a ſpring, which needs not be very ſtrong, cannot bruiſe its coats much; and their being oiled preſerves them from corrupting ſoon, as the metal of the plates and japanning of the ſpring prevent any bad conſequences from ruſt.

When this inſtrument is to be taken out, the plates are preſſed together; or, if it has been wholly lodged within the vagina, it is brought away by drawing the ſkains of thread. And the patient can with little or no trouble, introduce or remove this inſtrument at pleaſure.

XIX. *An Account of the ſides of the* Os Uteri *being grown together in a Woman with Child;* by the ſame.

A Woman, forty years of age, obſervably narrow between the *oſſa pubis* and the *os ſacrum*, had been four days in ſevere labour of her firſt child, when I was called to aſſiſt

 her:

her: The child appearing to have been dead for some time, I opened its head, and extracted it, but with great difficulty, its ſhoulders and haunches being too large to paſs in the ſtraitened paſſage between the bones. During ſome days after her delivery, ſhe paſſed a great many ſmall rugged ſtones by the urethra, and at length, after her urine had been ſtopped ſome time, her huſband drew out of the urethra a large piece of thick membranous ſubſtance, three inches in length, and in ſome parts two inches broad. One ſide of it was covered with a cruſt of ſmall ſharp ſtones, the other ſide was inflamed and bloody; which made me judge it to be part of the coats of the bladder ſeparated; and I was confirmed in this opinion, by introducing a catheter into the bladder; for, whenever it touched certain parts of the ſides of the bladder, blood came with the urine. The patient continued a long time with a plentiful ſuppuration about the pudenda; but we did not ſuſpect that the pus came from the internal parts, but only from the exterior, which had been ſomewhat lacerated.

About three months after her delivery, ſhe fell again with child, and took her pains after the ordinary period. She continued two days in hard labour before I ſaw her. The midwife then informed me, that the inner orifice had yielded nothing; I left her half a day, and, things remaining in the ſame way at my return, I examined her condition, and found that the *os tincæ* had not only not yielded, but that the ſides of it were grown together, without any veſtige of a paſſage; whereupon

I

I asked the assistance of another physician, and Dr Haddow being called, was, as well as the midwife, sensible of the case being such as I judged it to be. Wherefore we agreed to make an incision into the *os uteri*, but were first obliged to dilate the vagina sufficiently, that we might operate more securely. We had no *speculum matricis*, and therefore behoved to supply it by some other instruments. We tried to make the dilatation with a pair of long broad-bladed forceps, but they neither had strength to dilate sufficiently, nor did they keep the vagina equally open. After this we caused two pieces of wood, each three inches long, and two and a half broad, to be made concave on one side, and convex on the other, and of no more thickness than we thought would be sufficient to bear a strong enough pressure by the necessary dilatation. When these were finely polished, and besmeared with grease, I introduced them into the vagina, with the concave faces towards each other; then sliding in the legs of a *speculum oris* between them, and turning its screw, I separated the pieces of wood so far as we could see distinctly the cicatrice of the grown-together parts, and could have easy access to divide them, which I did by an incision, at least half an inch deep, before I pierced thro' the substance of this part of the womb; then, immediately introducing my finger at this wound, I touched the head of the child, and felt the whole circumference of the passage hard like a cartilage, which yielded nothing to several throws she had after the incision: So that I was obliged to guide a narrow bladed

ſcalpel with my finger, to make ſeveral inciſions into this cartilaginous ring. In doing this, there was not the leaſt appearance of blood, and the patient had no trouble, except what the dilatation of the vagina gave her. The labour continuing, the paſſage dilated a little, but not ſo much as to give any hopes of its allowing the child's head to paſs, notwithſtanding the bones of the cranium were overlopped; and, therefore, I was obliged to bring away this child as I had done the former. In this birth, there was no liquid with the child, nor did any blood follow it; it was quite ſupple, and had a white chalky cruſt over its whole body; ſo that we were convinced it had been dead ſome time.

The want of waters was ſome ſurprize, till I recollected, that, in the time of labour, ſhe told us they were paſſing, at which time I had the curioſity to make a ſtrick obſervation, and found, that what ſhe called the waters paſſed by the urethra, which opened externally by three different orifices: This, with her having loſt ſuch a portion of the bladder formerly, and her being ſubject to the gravel, gave me ground to think there was ſome communication between theſe paſſages and the cavity of the womb, above the *os tincæ*, which had allowed the waters to be evacuated. I was the more inclined to entertain this ſuppoſition, becauſe frequent inſtances have been obſerved of ſtones making their way through the neighbouring parts, as happened lately to a boy in this neighbourhood, who paſſed a very large ſtone, which had lodged long in the bladder by the anus, by which the

urine

urine had its courſe for ſome time after.

My patient, immediately after being put to bed, was ſeized with a pleuritic pain, very high fever, and difficult breathing; which coming on ſo ſoon after her being fatigued ſeveral days with hard labour, during which ſhe ſlept none, but drank much of every thing in the way, appeared to me rather the cauſe of her death, in twenty-four hours after, than any conſequence of the inciſions I had made; for ſhe never complained of uneaſineſs in the parts I had cut, nor had any hæmorrhage. Notwithſtanding all the ſolicitations I could uſe with her relations, I could not prevail with them to allow me to open her body.

## XX. *The Deſcription of a* Forceps *for extracting Children by the head, when lodged low in the* Pelvis *of the Mother; by Mr* ALEX. BUTTER *Surgeon in* Edinburgh.

THE forceps for taking hold of a child's head, when it is fallen ſo far down among the bones of the Pelvis, that it cannot be puſhed back again into the uterus, to be extracted by the feet, and when it ſeems to make no advances to the birth by the throws of the mother, is ſcarce known in this country, though Mr Chapman tells us it was long made uſe of by Dr Chamberlane, who kept the form of it a ſecret, as Mr Chapman alſo does. I believed, therefore, that a ſight of ſuch an inſtrument, which I had from Mr Dusè who practiſes midwifery

wifery at Paris, and who believes it to be his own invention, would not be unacceptable to you; and the publication of a picture of it may be of use to some of your readers.

Tab. V. Fig. 4. represents this instrument seen obliquely, of one third of its real dimensions.

A, is the extremities of the blades made more concave in the middle, than is necessary to fit them to the surface of the convex head of the child, in order, as Mr Dusè said, to hinder them to compress the temporal arteries.

B, is the convex side of the one blade.

C, the convex side of the other.

D, the hinge where the two blades cross.

E, a large flat button of a screw, which serves as an axis to the hinge, and can be taken out at pleasure.

F, a second hinge, by which the blades can be joined when the child is higher up than can be conveniently reached by the instrument, when the other hinge is employed.

G, G, the handles.

When this instrument is to be used, the axis of the hinge is to be taken out, and each blade, being directed by one hand in the vagina, is to be introduced separately along the side of the vagina, and betwixt it and the side of the child's head, as far as immediately above the ears; then, the two blades of the instrument being crossed, the axis is put into the hinge, which the operator finds most convenient to employ; after which the child's head is to be taken firm hold of, and the operator pulling by the handles, extracts the child.

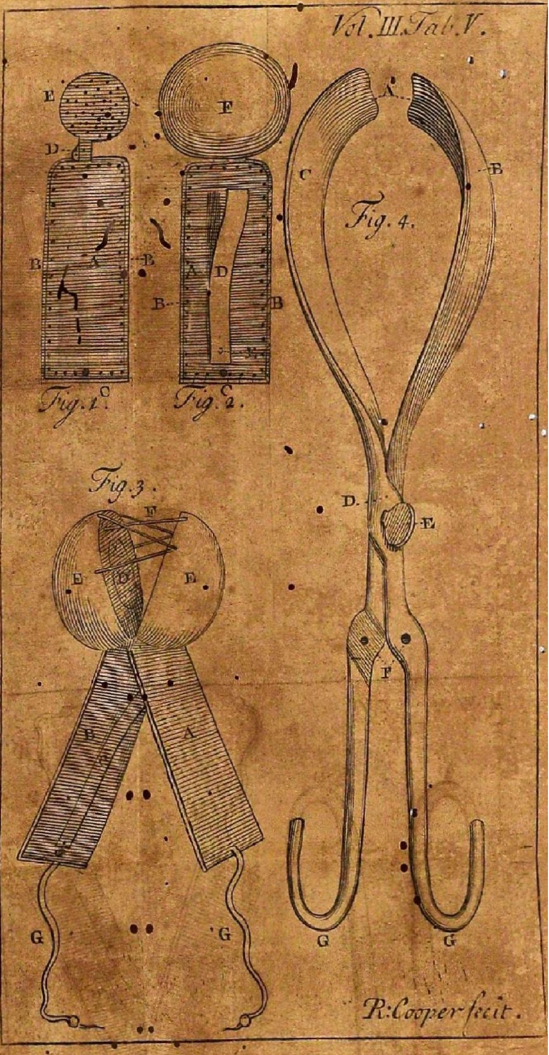
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Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
R: Cooper fecit.

I think Mr Chapman is in the right to desire the axis not to be put in; for it is very troublesome to take out and put in again, when any of the blades quit their hold, and the instrument can easily be managed without it, in extracting the child in the manner mentioned; and, in several cases where it may be requisite to dilate the lowest part of the passage at the same time that the extraction is making, the blades of the forceps require to be separated, and are not to be crossed or moved upon a hinge.

You will easily see, that often when the head of a child is a little too far forward on the *ossa pubis*, or turned too far backwards, that one blade only of this forceps can be employed to bring it to a right situation, and to assist the birth.

XXI. *An Account of a malignant* Lues Venerea, *communicated by Suction, in the City of* Cork, 1728; *by* EDWARD BARRY, M. D. *F. R. S.*

THE Venereal Disease, in the common way of infection, has been fatal to many. In this view, it demands as much attention as any disorder. But, if an accident of this nature, which I now transmit to you, is not guarded against, it may become a more general misfortune, and to such who least deserve it.

A woman in this city, who was commonly employed to draw the breasts of lying-in women, had probably, in the course of her business,

ſineſs, received the infection in her mouth; which ſhe either did not ſuſpect, or concealed, till ſhe had communicated the poiſon to ſeveral perſons of diſtinction.

I think this infection was ſtill more remarkable for its malignity, and the quick progreſs of the ſymptoms, than for the uncommon manner of its being received. As I had an opportunity of ſeeing moſt of the unhappy perſons who were thus affected, I thought that a faithful account of its appearance, and the method of cure, which I found ſucceſsful, might not be unworthy of a place in your eſſays.

The nipple firſt became lightly inflamed, which ſoon produced an excoriation, with a diſcharge of a thin liquor; from thence red ſpreading puſtules were diſperſed round it, and gradually ſpread over the breaſt, and, where the poiſon remained uncorrected, produced ulcers. The pudenda ſoon after became inflamed, with a violent itching, which terminated in chancres, that were attended with only a ſmall diſcharge; and, in a ſhort time after, puſtules were ſpread over the whole body. It finiſhed this courſe, with all theſe ſymptoms, in moſt perſons, in the ſpace of three months.

This diſorder made a quick and dangerous progreſs in ſuch who firſt received it; they not being apt to ſuſpect an infection of this nature in their circumſtances. The huſbands of ſeveral had chancres, which quickly communicated the poiſon, and produced ulcers in the mouth, and red ſpreading puſtules on the body: But ſuch of them eſcaped who had timely notice of the

the nature of the disease, before the pudenda were affected. Some infants received it from their mothers, and to the greatest part of them it was fatal.

When I first mentioned my opinion of this disorder to the midwife of a person whom I visited, she said, the woman who drew her breasts was a few days before, on such a suspicion, examined, at the request of a lady of distinction, before she would make use of her; and was declared free from any such distemper; by which means that lady was unhappily deceived, and was one of the last who received the infection. I ordered the woman to be sent to me, and observed a small ulcer at the root of her tongue, and a large recent cicatrice on the inward part of the upper lip. She obstinately denied that she ever had any sore there, but was so much terrified, when I told her she would certainly rot away, that she begged I would not suffer her to perish, if I suspected so much danger. While she was in a salivation, she owned to me and Mr Osborne, an eminent surgeon in this place, that she had an ulcer where the cicatrice remained, which she cured by two or three doses of physic, and a gargle made of woodbine, and some other ingredients, and said she concealed it, because she imputed it only to cold, and was afraid, if known, it might for ever destroy her business and character.

This woman, who communicated this infection to so many, had no eruptions on her body; and, by what I could find, the infection never made any progress beyond the mouth. May not

not this be accounted for from the particular way of life which ſhe followed, the humours being diſcharged by an almoſt conſtant ſalivation, and the parts deterged and cleanſed by a gargle of breaſt-milk?

Some women, whoſe breaſts were drawn by her, had never any marks of infection; but, by what I could find, the few who eſcaped were ſuch whom ſhe attended after the large ulcer on her lip was healed; for, while that continued, the nipple was received into a bed of corruption. But the caſe of a lady was very remarkable, whoſe breaſts were drawn twice a-day by her, when ſhe communicated the infection to all others who came in her way. A violent confluent ſmall pox ſeized this lady immediately afterwards; and ſhe never had any marks of infection.

The activity of this poiſon was ſo great, that I immediately directed a mercurial ſalivation to even ſuch as were but lately and lightly affected, and ordered it to be brought on by repeated unctions, in a ſmall quantity, with a few grains of calomel internally, and continued the ſalivation five or ſix weeks.

I have often obſerved, that, where the ſalivation eaſily riſes on the uſe of the ſmall quantity of mercury, the cure is uncertain, and the ſymptoms often return: And that the ſame inconveniencies often attend a large ſalivation accompanied with a great inflammation, from which many other dangerous ſymptoms alſo flow.

The firſt of theſe is more frequently the caſe of women, and ſuch whoſe fluids are naturally

ly diffolved, and whofe veffels are relaxed and tender. The other attends perfons of a reverfe conftitution. In the former, mercury paffes off too quickly through the larger veffels, without penetrating into the fmaller canals, where the feat of the diforder moft frequently lies. In the other, the great vifcidity of the humours oppofes their diffolution, which muft be always previous to a free and effectual falivation; and is abfolutely neceffary to difengage the infection from them: So that it may eafily, by a proper determination, be difcharged from the body: For, whenever the animal fluids are heated beyond their natural degree, they immediately become vifcid; and all violent inflammations being attended with this effect, muft therefore counteract the operation of mercury. Previous warm bathing, gentle evacuations, if there is a plenitude in the veffels, and a diluting regimen, are neceffary to prevent thefe evils; and, after fuch preparation, a lefs quantity of the medicine will produce the defired effect with more eafe and efficacy.

In the lax habit, neither is bathing, nor much diluting neceffary, till the falivation appears; but the medicine muft be repeated at a proper diftance, and in a fmall quantity, to anfwer this end; though, in fome cafes of this nature, it may, with more fuccefs, be determined to the furface of the body, and made fudorific.

The venereal diforder returned to fome after a regular falivation, but was entirely removed by the following method, which I made

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uſe of to all who had this infection in a violent degree.

I ordered a machine to be made of oak, the cavity of which was four feet long, and ſixteen inches deep, that a perſon might ſit in it with his legs extended; a cover moving in a groove was ſo adapted, that it cloſed every part round the body. When this machine was to be uſed, it was filled to about the height of eight inches, with a ſtrong bath of herbs, with *ſal. gem.* diſſolved in it.———The proceſs was as follows.

I directed them to take in the morning and afternoon, in divided draughts, a quart of the following decoction.

℞. *Rad. opt. bardo* [illegible] *ſarſaparil. Chinæ glycyrrhiz. an. unc.* ii. *cum aq. decoct. ſpatio ſem. horæ. Adde ligni raſi ſantal. rubri, ſaſaſras an. unc.* ii. *Ebulliant iterum parum, dein decoct. lib.* viii. *exhibe.*

In the evening, about an hour after they had taken the decoction, they went into the bath, as hot as they could well bear it; the lower part of their body being naked, the reſt, and eſpecially the head, being well guarded from cold. The ſteam, which had no other paſſage but where the opening was allowed for the body, and the heat of the bath, in a few minutes, threw them into a very profuſe ſweat. They ſeldom remained in the bath above an half hour; when they came out of it, they were well rubbed near a large fire, dry linen was put on, and they went into a warm bed, where they continued ſweating two hours. During this time, they drank very plentifully of

of three parts water, and one of milk warmed; their linen was again changed; they fat up for two hours, and eat a light fupper of bifcuit, with broth or fack-whey. At dinner, I allowed them any of the white meats, with fack and water for drink.

When they had bathed in this manner five or fix times, I ordered a ftrong decoction of guaiacum to be ufed inftead of the former, and a few grains of calomel to be taken an hour before they entered the bath; and in fome cafes I have directed two grains of the turbith mineral to be mixed with the calomel; which, though continued for a confiderable time, feldom affected the glands of the mouth, being determined with fuch force [illegible] of the body. And, if the expreffion may be allowed, mercury thus managed, produces a falivation through the pores of the fkin. When the falivary glands are in the leaft affected, the ufe of mercurials is to be omitted, till that fymptom difappears.

I have fometimes obferved, that the mercurial, taken going to reft the night preceeding the ufe of the bath, is lefs liable to affect the ftomach, than when taken an hour before bathing.

I ordered the bath three times in a week; in fome cafes repeated it five or fix times fucceffively every day, and gradually remitted the ufe of it.

The patients were feldom faint with fweating, a large fupply of milk and water, thin gruel, and fometimes fack whey, preventing this inconvenience. During the whole courfe, they

 had

had a very good appetite, and more than uſual chearfulneſs of ſpirits. I have known ſome in deſperate venereal diſorders, reduced by former methods, acquire a good complection and ſtrength in this courſe; and, though I have uſed it in many advanced and dangerous caſes, I never met with one unhappy accident attending it, or with any inſtance where it failed.

I have of late given *mercurius præcipitatus per ſe* in ſome caſes, inſtead of the former preparations, as being leſs liable to affect the ſalivary glands than any other preparation of mercury. I have frequently directed a grain and a half of it twenty nights following in a common unguarded way, without obſerving that it affected the gl[illegible]d any remarkable ſenſible evacuation: but lately the ſame quantity, taken ſeven nights ſucceſſively, raiſed an unexpected and violent ſalivation in a patient of mine. This, however, has given me a greater opinion of the efficacy of this medicine.

## XXII. *A remarkable* Hydrocephalum; *by Mr* JAMES MOWAT, *Surgeon at* Langholm.

A Child of a year old is rather leſs and ſmaller than ſhe was a month after ſhe was born, having ſo little fleſh on her bones, that ſhe is almoſt a very ſkeleton. She has no colliquative ſtools to keep her from growing, but is only dull and lethargic. She has two teeth in the under-jaw, and two appearing in the upper. When ſhe was a month old, her head began

began to ſwell, and continues yet, daily to increaſe; it is now twenty-ſeven inches and a half in circumference; from the point of the noſe (which is very much depreſſed in the middle) to the nap of her neck, twenty-one inches and a quarter; from the one ear to the other, I think it is about eighteen inches. The face above the eyes is exactly ſix inches broad; the cheeks and mouth are very ſmall for want of fleſh; the eyes are large, and ſhe can hide them totally within the orbit, either above or below. All the bones of the face and *oſſa temporum* ſeem to be very large, firm, and broad, without any future till near the open of the head; but the reſt of the head is like a ſoft quagmire, covered all over with a large, [illegible] ſcab. The veins upon the face, and where the ſkin is free of the ſcab, are very large, full, and blue; ſo that they are viſible in their minuteſt ramifications. The child takes no food but the mother's milk: She keeps the head of the child always lying in a hollow between two pillows.

XXIII. *A* Hydrocephalum *with remarkable Symptoms; by Mr* JOHN PAISLEY, *Surgeon* in Glaſgow.

A Boy betwixt ſix and ſeven years of age, of a ſound conſtitution to appearance, and who had been very healthy from his infancy, was ſuddenly ſeized one morning with a pain in the left ſide of his head, attended with an unuſual drowſineſs and laſſitude; which increa-

 ſed

fed in the afternoon; his pulfe not very qu[illegible]k, fhort cough, loathing all kinds of meat and drink, an inclination to vomit, with a flufhing in his face at times, other whiles pale and ill-coloured, and pains in his belly; the gums of his four back jaw-teeth much fwelled, and his mouth hot.

It being fufpected that worms were the chief caufe of his diforder, fome worm-powders were given him, clyfters injected, and his belly rubbed with the *unguentum vermifugum.*

The firft three days he continued much the fame way, being always more brifk and lively in the forenoon, but very dull and heavy in the afternoon. On the fourth day he complained more of the pain in his head, [illegible]ing a little more feverifh, he was let blood [illegible] the jugular, to betwixt four and five ounces, had a clyfter injected at night, and next morning got a vomit of ipecacuanna, which operated very well: At the fecond puke, he brought up a pretty large live worm, five or fix inches long, of the teres kind; by this he feemed to be fomewhat eafier, and more lively all that day.

On the fixth, he got fome more dofes of worm-powders, though it was with great difficulty he could fwallow them, having an averfion to either meat or drink, as well as to the powders; a vermifuge plaifter was applied to his belly, and a clyfter injected.

On the feventh, his pulfe was rather flower than the natural; and though, as was obferved, he ufed to be more quick and lively in the forenoon, yet this morning he was fo drowfy and dull, that it was with great difficulty he was

prevailed

prevailed on to take a purgative potion: It was much ſtronger than any he had ever taken, yet had no operation; wherefore he got a ſtrong purgative clyſter, by which he had only one ſtool, being a diſcharge of ſome hardened fæces, covered over with a kind of mucus or ſlimy ſtuff, and with them a large worm like the former, nine inches long, came away.

On the eighth he was ſo dull and ſleepy, that he could be diverted no manner of way, nor prevailed upon to take any aliment, and far leſs medicine: The purgative clyſters continued to be injected, never gave him above one ſtool. In the afternoon, he ſeemed lethargic, his pulſe ſlow and unequal: A clyſter made of the tops of wormwood and leſſer centaury boiled in claret, was injected, and a bitter infuſion in wine was ordered to be taken by the mouth; theſe raiſed his pulſe a little, and he began to take ſome panado, or other ſoft aliment, which he did in a very quick manner, ſtaring very broad, and without ſpeaking a word, but could not be prevailed upon to taſte any kind of drink; and, ſo ſoon as he had done, immediately fell aſleep again.

On the ninth, he was much as the day before, only more comatoſe: A bliſtering plaiſter was applied to his neck, which roſe very well, and diſcharged a good quantity of ſerum, without in the leaſt rouzing him, or making him more ſenſible, only his pulſe was a little quickened.

On the tenth, he was much as the day before, only his face was very florid and red, and ſomewhat ſwelled; and he frequently put his hand

to

to the left ſide of his head, breathing a little quicker than uſual, with a wheezing and ſome difficulty; his pulſe ſlow and languid: Upon which his head was ſhaved, and the part where he complained the pain was at firſt, and to which he often put his hand when he could not ſpeak, though there was no appearance externally of any ſwelling, was ſcarified and cupped, and three or four ounces of blood drawn away: This eaſed him as to his breathing, and removed the redneſs and ſwelling of his face, but produced no other viſible effect; ſuppedalia were applied at night.

On the eleventh, his pulſe was exceeding ſlow and intermitting, his coma ſo much increaſed with ſtartings, th[illegible]ake no kind of aliment, and appeared [illegible] no manner of ſenſe; and thus he continued perfectly inſenſible till next forenoon, when he died.

All along he had ſuch a heavineſs in his eyelids, that he could not lift them up with eaſe, and they appeared ſwelled.

Upon opening his head, ſo ſoon as the cranium was removed, I obſerved a protuberance or tumor about the bigneſs of a large haſel-nut, on the *dura mater*, under the parietal bone of the left ſide, about an inch and a quarter from the ſagittal ſuture, and about two inches from the lambdoidal, which was the place the child had pointed his pain was: This tumor had made no apparent pitting in the bone; it felt ſoft, and, upon opening it, there iſſued out a little bloody ſerum, and in the bottom of it I diſcovered a great number of little white bodies like the ſmall worms in blown meat, or the

ſubſtances

fubftances fqueezed from perfons nofes; they had no appearance of life, and were contained in a duplicature of the *dura mater*; the inferior fide of which at this part adhered fo firmly to the *pia mater*, that it was impoffible to feparate them without lacerating thefe tender parts. There were feveral other fmaller ones along the left fide of the *finus longitudinalis fuperior*, befides the common luxuriant rifings of the brain that are always obferved in this part; thefe contained the fame kind of bloody ferum and white bodies as the large tumor: Whether they were real worms, or an obftruction in the glands of the *dura mater*, following fome inflammation in thefe parts, I could not determin[illegible] incline to the laft; fince, where-ever they were, there was fuch a ftrong adhefion of the *dura* and *pia mater*, that they could not be feparated without lacerating the tender parts below. All the veins in the head were turgid with blood, as if they had been finely injected, though there was little or none in the other cavities when opened.

When, in taking out the brain, I had cut the optic nerves, I obferved a preternatural thicknefs of the *pia mater*, and a large diftenfion of it as with water; upon cutting it open, about half an Englifh pint of a yellow-coloured water iffued out. In diffecting the brain, I found the ventricles had been much diftended and enlarged by the water; the *plexus choroeides* were hard and fchirrous, with a great number of fmall hydatides (as I fuppofed) lying along them in rows, whofe coats were

were exceeding tender, and burft upon the leaft touch; they exactly refembled the lymphatics delineated in the fifth table of Dr Ridley's Anatomy of the Brain.

I obferved little that was remarkable in any of the other vifcera, they being all perfectly found, except the inteftines, which were empty, and in feveral places inflamed, with apparent figns of a beginning mortification: There were fome few of the teres worms in them that appeared dead, moft of the fmall inteftines being tranfparent, and in two places one part was drawn up confiderably into the other, like the finger of a glove, as if the periftaltic motion of the inferior part had been inverted, while that [illegible] fuperior part continued, and fo the one thruft into the other: It took a confiderable force to draw the one out of the other, the inteftine being much contracted in this part, though there was no fign of any inflammation in thefe parts.

Since this cafe was under my care, I have feen feveral children who complained of a pain in one particular part of their head, having a great drowfinefs and heavinefs in their eyelids, a pulfe much lower than the natural, no drought, a great averfion to food or drink, an inclination to vomit, and other fymptoms, as this boy had, which made me judge them to labour under the fame difeafe; and the diffection of two, who were all I was allowed to open, fhewed my opinion to be juft, the parts being found affected in much the manner above defcribed, only that I could obferve in neither of them any thing like the little tumors mentioned in

in the former case; and in the latter two, all the vessels of the *plexus choroeides* were hard and obstructed, and the *pia mater* at the bottom of the brain, immediately under the optic nerves, was so considerably thickened, as to appear almost like the *dura mater*.

XXIV. *An uncommon* Angina; *by* ALEX. MONRO, *Professor of Anatomy in the University of* Edinburgh, *and* F. R. S.

A Man thirty four years of age, subject all his life to plethoric indispositions, especially in the spring, and to catarrhs when the least exposed to cold, had not used any fermented drink, or heating food for several years, on account of a slight hæmoptoe that had more than once seized him, and had retrenched some of his ordinary diet for near two months, to prevent his vernal plethora; having sat several hours in a chamber without a fire, and with an open window, while the weather was very cold and the air foggy about the middle of January 1732, complained, as soon as he rose from his seat, of being stiff with cold and very weary, with pains in moving all his muscles, and perpetual yawning: To free himself of the uneasy coldness, he immediately went home, sat near a fire and drank tea, but could not remove the shiverings and weariness. Upon going into bed he was seized with the appearance of an aguish paroxysm, which had made a regular course before morning.

Next

Next day he was a little feveriſh, and ſtill complained of pains through all his body, and therefore took his uſual cure in ſlight maladies of that kind, *viz.* a laxative ptizan in which tamarinds and ſenna had been boiled; which operated gently, and gave him conſiderable relief.

The day following, he was almoſt free of all his former complaints, but his right amygdala was ſwelled and painful, for which the ptizan was repeated.

In the morning of the fourth day, the amygdala was leſs and freer of pain, and, except a little bitterneſs in the mouth, he had ſcarce any other uneaſineſs. But in the afternoon, the left amygdala became painful on the leaſt attempt to ſwallow, his pulſe turned quick, but not very ſtrong: He was let blood at a large orifice of a big jugular vein, to the quantity of ſixteen ounces; after which his pulſe became very quick, ſtrong, and full: On this appearance of plethora, eight ounces more were taken away, and the patient then inclined to faint; but, in a quarter of an hour after, his pulſe was very near as ſtrong and quick as ever. The pain of the left amygdala was ſo far from being diminiſhed, that it continued increaſing, and all the teeth in the ſame ſide of the lower jaw, that were otherwiſe very ſound, were alſo violently and conſtantly affected, but the pains were moſt exquiſite when he ſwallowed. In the mean time, he could open his mouth as wide as ever, and nothing preternatural could be obſerved in his fauces, except a ſmall ſwelling of the right amygdala, and an

eryſipelatous

erysipelatous redness of the uvula, *velum pendulum*, and left amygdala, without any tumor. After he went to bed, the pains were so racking, especially when he swallowed his spittle, (which he could not possibly prevent doing frequently, whatever care he took to the contrary), that, though he is a man who is tolerably patient under pain, he made the whole bed shake with his tremblings, and large drops of sweat appeared on his skin at each attempt. About midnight, he could lie no longer; but, putting on his cloaths, he received the fumes of warm water into his mouth; and the pain remitting somewhat, while he endeavoured to hinder the occasions of its violent increase, by leaning his head forward, opening his mouth, and so allowing the saliva to run out, he passed the rest of the night in a drowsy nodding way.

Next morning, his deglutition was performed with great difficulty and pain; and he was so hoarse, he scarce could be understood when he spoke, but had no difficulty of respiration; and the fulness and strength of his pulse were less, but its quickness remained. His throat had the same appearance as the night before. The purgative ptizan was repeated. The fumes of warm water, were often made use of, and an emollient cataplasm was applied externally on the pained parts.

In the evening, the pain was easier, and the pulse rather better, but the hoarseness continued. A blistering plaister was applied on his neck and back, and emulsion was prepared for his drink.

 The

The plaister did its office well, and the patient, notwithstanding his pain in swallowing, drank two bottles of emulsion in the night, which prevented any strangury till near noon of the following day, when, the chamber being overheated, and the patient thereby put into a sweat, the strangury came on violently; of which he was relieved, after some hours, by carrying away the fire, and injecting an emollient clyster with turpentine. The emollient poultice was in the mean time renewed, and the fumes of water were frequently applied, and a mild resolvent gargarism assisted the evacuation of mucus, which began now to be secerned in more than ordinary quantity in the fauces. In the evening the patient seemed by his pulse to be pretty free of fever, and all his troublesome symptoms were considerably removed.

The blistered parts discharging plentifully the two succeeding days, he seemed to mend apace; but still continued the poultice, fumes, and cataplasm.

On the third, which was the ninth of his disease, finding in the morning the pain increased, he took the laxative ptizan, and plied the topical medicines; but through the day the symptoms gradually increased, till, in the evening, his pain in the left amygdala and teeth was rather more violent than ever; the hoarseness was worse than formerly, his pulse was low and quick, he was drowsy, and passed little urine. No swelling could be seen on the left pained side, and he breathed freely. He drank largely of warm posset made with Rhenish

wine,

wine, and chewed long pepper with the teeth of the affected ſide. His urine ſoon came in great plenty; he diſcharged large quantities of mucus at the mouth, and in two or three hours was much freer of pain; his pulſe turned ſlower, and the dozing went off. The gargariſm was renewed, with the addition of ſome *aq. theriac.* and *ſp. nitr. d.*; and *theriac. andromach.* was added to the cataplaſm.

By the uſe of theſe medicines, he was greatly recovered in the two following days, and nothing remaining the third, except a little bitter taſte, a trifling ſwelling in the right amygdala, and a weakneſs in the left ſide of the fauces: He went abroad to his uſual labour of diſſecting in the forenoon, and preſecting in the afternoon, which he continued to do for a week, uſing a ſpare diet and applying the topics; in which time he got quite free of all uneaſineſs, except a dull pain of the left ſide of the fauces, eſpecially in yawning; and he imagined all the food he ſwallowed paſſed only by that ſide, where ſometimes a little of it ſtopt, and was with ſome trouble preſſed back again into the mouth.

To remove this weakneſs, the patient one evening gargled his throat with claret wine and a little *aq. theriac.* mixed. That night, he was ſenſible of a ſtraitening in his breath; and in the morning when he awaked, he was quite hoarſe, breathed with more difficulty, and, in coughing, his head and eye-balls felt as if ſtrongly girded, his face became very red, and he began to hiſs in breathing. All theſe bad appearances went greatly off, after breath-

ing in the fumes of warm water, and drinking warm tea. In a few hours after, he swallowed pills composed of *mercur. d.* and resine of jallap, which operated mildly, and lessened all the causes of complaint considerably, and a second such dose, taken two days after, removed all of them entirely, except a little weakness of the left side of the fauces, and an obtuse pain there in yawning, which remained several weeks after.

In the history of this disease, I have given a strong hint who the patient was, and suspect the common frailty of enlarging on one's own misfortunes may have discovered sufficiently, that it is my own case I have related.

What was the particular seat of this anomalous angina? Will the uses assigned to the digastric muscles in Art. XI. of Vol. I. be of use to discover it?

## XXV. *An* Asthma, *with uncommon Symptoms*; *by* the same.

MR James Ure, whose business as a writer, or solicitor at law, engaged him frequently in company and good fellowship, was sometimes troubled with a palpitation at his heart, a trembling in his hand, and faintness; being always subject to a cough, and having generally very little appetite for food, with two or three loose stools a-day. In October 1730, the fifty-fifth year of his age, having catched cold, a cough seized him, with which he brought up a small quantity of very thick slime, and complained

plained of a difficulty of breathing; he lost all appetite for food, and his belly became bound; his urine was in small quantity, and he could not feel his own pulse.

He allowed these symptoms to continue eight days without asking any advice; but, finding them continue, and rather increase, he consulted Dr William Porterfield and me, October 20. Besides the above-mentioned symptoms, we observed his difficulty of breathing increase so much upon lying down, that he was obliged to sleep in a sitting posture. His feet and legs were very oedematous. We could feel no pulse in the arteries of his wrist, neck, temples, or ham, but only a sort of trembling under our fingers, which we then attributed to some irregularity in the distribution of his arteries, notwithstanding his affirming, that, when in health, he had a pretty strong pulse in the very parts we felt. The veins of his arm and neck were very large, and stretched with blood. He could take no sort of food, but had a thirst, which was not violent, nor was his tongue dry, or his skin hot. When the cough attacked him, he forced very little pituit up; but his breathing became exceeding laborious, his face turned very red or purple, and he complained of a violent head-ach. When he was free of the cough, a little while he walked, spoke, complained of little, and assured us he was scarce weaker than in health. To prevent the bad effects of the cough, twelve ounces of blood were immediately let; he bled freely, and did not turn faint. In the evening, a terebinthinat purging clyster was injected,

ed, which purged him gently. He was desired to take any sort of mild food which his stomach could receive. Rhenish wine and water were given for drink, into which he frequently dropped forty guts of the following mixture: ℞. *Elixir pectoral. unc.* i. *Tinctur. castor. drach.* ii. *Sp. salin aromat. drach.* iii. and every two hours he swallowed two spoonfuls of the *oxymel pectoral.* of the Edinburgh dispensatory. At bed-time, he took this bolus: ℞. *Sperm. cet. scrup.* i. *Sal. succin. gr.* v. *Conserv. ros. scrup.* i. *Syrup. alth. q. s. ut fiat. bol.*

Next morning, there appeared no change in his condition; and he took *p ipecacuan. gr.* xxxv. *Oxymel scillit. unc.* i. This emetic operated six times, without increasing his headach or difficulty of breathing, and brought a good deal of slime with the water he drank. He seemed a little relieved after this vomit, and continued the use of the medicines prescribed the day before. At night, a blistering plaster was applied to his neck and shoulders; he took his bolus, and had a cooling emulsion for drink.

The blister rose well, but the pain of it made him so unable to move next morning, that we could not give the purgative we had determined to order. His urine was in larger quantity, with a good sediment; notwithstanding which, his legs were rather more swelled, and the other symptoms continued. His bolus was again given in the morning; he still drank water with Rhenish wine; and he was desired to take frequently a spoonful of the following

following mixture, ℞. *Aq. hyſſop. puleg. a. unc. iii. Cinam. ſ. v. raphan. compt. a. unc* i. *Gum. ammoniac. drach.* ii. *Sal ſuccin. drach. ſem. Oxymel. ſcillit. unc.* ii. *Syrup. alth. unc.* i. *M.*

He continued much in the ſame way all the 22d and 23d, only his legs and thighs ſwelled more; and he began to complain that his cloaths were too tight for his belly.

In the morning of the 24th, he took this purgative bolus, ℞. *Pulv. rhei elect. ſcrup.* i. *Aquil. alb. pulv. jallap a. gr.* v. *Syrup. de rhamn. q. ſ. ut. ft. bol. capt. é ſyrup. violar.* This purged him mildly, and the quantity of his urine increaſed conſiderably, and his belly and legs became leſs ſwelled, which relieved him ſomewhat of the orthopnea.

Next day, he began to uſe a medicated wine: ℞. *Rad. irid. Florent. ſcill. acor. ver. helen. raphan. ruſt. a. unc. ſem. Gentian. drach.* ii. *Rub. tinctor. unc. ſem. Cortic. ſambuc. ebul. a. unc. ſem. Herb. marub. alb. m.* ii. *Abſinth. rom. centaur. min. a. m. Sem. bacc. juniper. unc.* i. *ſem. Sal. tartar. drach* iii. *Inciſ. & contuſ. ſ. a. M. ut ft. malia infundend. in vin. Liſbon lib.* vi. *Colatur. capt. unc.* ii. *mane & hora 5ta veſpertina.* All this day and the following, his urine paſſed plentifully, with a large ſediment; the ſwellings diminiſhed, and his cough and breathing were eaſier. In the evening of the 26th, while I ſat by him, he ſurpriſed me with putting his wriſt into my hand to feel his pulſe, which was full, ſtrong, ſlow, and equal; but, two hours after, I could again

again feel no more than the very weak trembling I formerly mentioned.

The 27th the purgative bolus was repeated. On the 28th, his pulse returned, and continued afterwards firm and strong, till he recovered his former health, which he did in a little time, by the use of the medicated wine, pectoral mixture, and the purgative sometimes repeated.

About a month ago, having got the cold again, his symptoms began to return with the same state of his pulse; but, upon taking the former emetic, and the purgative, they went off; and, to prevent a second relapse, he got strengthening chalybeat medicines, with the stimulating gums, for some time.

In the beginning of January 1732, he was again attacked with the asthma, and want of pulse; which were removed with two doses of the purgative. After a cough, which continued some days in the beginning of February, his pulse could not be felt during twenty-four hours, but it returned after the operation of a purgative bolus.

Soon after my patient retired to the country, where he has continued in very good health; and being lately in town, revised this account of his case, and allowed me to communicate it to you.

XXVI.

## XXVI. *A large* Steatom *passing with the* Oesophagus *from the* Thorax *into the* Abdomen; *by Mr* JAMES JAMIESON, *Surgeon at* Kelso.

A Carpenter in this place, thirty-six years of age, of a thin make of body, asked my advice about the middle of February 1732; his complaints were, pains at the heart, in the pit of the stomach, both sides of the thorax, and betwixt his shoulders; a great difficulty in swallowing, and frequent inclination to vomit. He told me it was about six years since, upon a sudden grief, he was first seized with the pain, and frequent palpitations at his heart; and that the other symptoms came on gradually afterwards, but never kept him from his ordinary work till now.

At my first visit, I took twelve ounces of blood from his arm; the blood was sizy like that in a rheumatism. Next day, he took *pulv. Ipecacuan. drach. sem. Tartar. emet. gr.* ii. which purged him four or five times, but did not occasion the least nausea. I gave him also doses of the *pillul. gummos.* twice a day, washing them down with *decoct. amar* in which a small quantity of *sal. absinth.* was dissolved; caused the pained parts to be fomented with a strong decoction of the aromatic plants, and applied a large plaister of *theriac andromach. ol. mac. per express.* and the powder of the carminative seeds. The blooding gave him some remission of pain, and was repeated twice in the

two following weeks, with some short relief each time.

Finding, however, his complaints rather increase, I obliged him to call a physician, which his poverty had made him obstinately refuse to do hitherto: And, while Dr Abernethy was sent for, he shewed me a hard painful tumor, immediately below the *cartilago xiphoides*, which, he said he had only discovered the preceding night.

The doctor suspecting, from the account the patient gave of his disease, and from the appearance of this tumor, that an abscess was forming in the stomach, ordered him a milk diet, and lintseed tea, or a decoction of *hedera terrest.* or milk and water for ordinary drink, and caused an emollient cataplasm to be applied to the tumor.

He continued to turn gradually worse, till about the middle of March, when he was attacked with a vomiting, which none of the different stomachics nor opiats given him could restrain, but were thrown back again, as soon as swallowed. His thirst became excessive and perpetual; but all his drink, though taken in very small quantities at a time, was vomited as quickly as his medicines, without increasing his pains; and a singultus soon came on, which continued till his death.

He passed very little faeces by the anus, and these were generally procured by gentle clysters.

His urine was for the most part limpid; when there was any sediment, it was white.

During

During the whole courſe of this diſeaſe, the patient's pulſe was never quick or ſtrong, but low and languid, and ſometimes intermitted. His pains were always moſt ſevere in the night, and at laſt deprived him almoſt entirely of ſleep; and, for a whole month, he did not lie down, but ſat in the bed, or on a chair, bended forwards with his head on a pillow. In this melancholy condition, he languiſhed till the firſt of April when he died.

On laying open the thorax and abdomen, we obſerved a prodigious large ſteatomatous body, which filled a great part of the thorax, being grown to the pleura in both ſides, to the larger ſhare of the mediaſtinum and pericardium; and then accompanying the oeſophagus, they paſſed together through the diaphragm, having enlarged the paſſage in this muſcle conſiderably, and ſtraitning the oeſophagus. After entering the abdomen, it was ſtretched along the dorſum of the ſtomach to the pylorus, which it alſo compreſſed greatly: Both the orifices of the ſtomach being ſo much contracted, that I could ſcarce puſh my finger through either. I was about to cut out this whole tumor, to meaſure and weigh it, but was oppoſed by the relations; and therefore behoved to content myſelf with examining its ſubſtance as it lay in the body. It was ſo hard, my knife could ſcarce cut it, but, after a tranſverſe inciſion, ſeveral ſinuſes, formed in the firm white ſubſtance, diſcovered themſelves; ſome of them contained a matter like a meliceris, in others, it was a-kin to that of the atheroma, and in a third ſort it was purulent and foetid.

The

The fat of the omentum was all wasted, but all the other viscera were found enough.

XXVII. *Of the Service of a warm Bath in a bilious Colic;* by ROBERT PORTER, M. D. *Member of the College of Physicians,* London.

IT is not my design to describe the several kinds of colics, their contradistinction from each other, and their different methods of cure; my purpose being to recommend one particular method of managing the bilious colic, and those severe ones, whose cure depends on procuring an entire thorough discharge of that acrid matter within the intestines, that causes the disease (though not properly bilious, because unattended with such vomiting) which seems but little attended to; yet will appear from reason and experience to be so highly useful, and so absolutely necessary, that this distemper ought never to be treated without such additional assistance.

The description of this disease by Sydenham * is so just, that in this point nothing can be added to him. It will also readily be confessed, that the grand indication of cure is to obtain an open passage through the intestines for a perfect discharge of that acrid irritating matter contained within them. As this therefore is the true cause of the disorder, to the grand point of its evacuation should the whole method be directed.

But

* Sydenh. § 4. cap. 7.

But ſuch is the alteration produced in the cavity of the bowel, by the conſtant ſharp ſtimulus of this acrid matter, that it is not only contracted into an unuſual narrowneſs; but, if the obſervation of authors of unqueſtioned veracity may be credited, the coats of the affected inteſtine have been found, upon diſſection, ſo cloſely joined, and ſo entirely precluding any downward paſſage, as if they had been ſtrongly girt round with a ligature.

But the circumſtances of the ilium bowel, produced by this ſharp irritation in theſe colics, will be perfectly underſtood, and cannot poſſibly be better explained, than by tranſcribing that moſt beautiful paſſage from Peyerus, where he relates an experiment he made on a living frog, that happily ſets this matter in the cleareſt light; not only viſibly explaining the effect of a ſevere colic on the inteſtine, but repenſenting, at the ſame time, the manner in which its frequent direful conſequence, the iliac paſſion, is produced.

" Torminum geneſin, atque introſuſceptionis
" conceptionem, anni ſuperioris æſtate, ranæ
" jucundo admodum et utili experimento oculis
" noſtris exhibuerunt. Etenim inteſtina, vi-
" vente amphibio, laceſſita in diverſis locis, per-
" tinaciſſimè mox ſe conſtringebant, contentis
" violenter furſum deorſumque, quâ data porta,
" protruſis, atque hinc inde in cumulos quaſi
" congeſtis: Unde quædam inteſtini portiones
" valde impletæ ac turgidæ, quaedam prorſus
" inanes et arctiſſimé clauſæ perſtiterunt; donec,



* Peyer. de Gland. inteſtin. cap. 9 p. 81.

"ſoluta fibrarum ſtrictura, meatus liberior r-
"deretur: Inteſtinis autem hinc inde ſe con-
"ſtringentibus, et ſive chylum, ſive fæces ſur-
"ſum deorſumve projicientibus, factum, ut
"illæ alicubi in molem adgeſtæ parietes inte-
"ſtini plus debito ampliarent: Quod in ſacci
"morem dilatatum intra ſe recepit conſtrictam
"inferioris inteſtini portionem, eamque ſinu
"ſuo abſconditam aliquamdiu retinuit: Donec,
"fibris ſe denuo exporrigentibus, inteſtinum,
"è latibulo alterius, in apricam priſtinamque
"ſedem rediret."

If ſuch then is the bowel's contraction in the ſeverity of this diſeaſe, one grand important point to facilitate this evacuation, on which the cure muſt turn, ſeems to be the removal of that ſtricture; and, by relaxing the tightneſs of the inteſtine, to procure a freer and wider paſſage for the diſcharge. For, unleſs this be jointly endeavoured, in conjunction with the other method of cure, I apprehend we do not aſſiſt the patient with all that our art might contribute to his relief. Nor do I know any thing ſo effectual to this deſirable end as the warm bath, which is daily found ſo highly advantageous in ſomewhat a ſimilar caſe, I mean the diſcharge of gravel from the kidneys. I am not ſo fooliſhly prejudiced to imagine the bath alone would prove effectual, or ought ſingly to be confided in for the cure of theſe colics: I hope I ſhall not be ſo underſtood; the whole I would endeavour is to propoſe and prove it a very efficacious aſſiſtant, joined with the other known and uſual methods, and what will greatly forward the happy effect of them.

But

But, since I have mentioned a nephritic disorder, as somewhat a parallel case, I beg leave to run over the several indications of cure in that disease, from whence the near similitude of managing both will readily appear; and the advantage of the warm bath in one be no small encouragement to its use, and evidence of its service, in the other; because the manner in which it is useful is in each the same.

It will be readily confessed, the grand point here is the evacuation of that sabulous matter lodged in the pelvis of the kidneys, or stuffing up the ureters. The methods to effect this are, immediate bleeding, to remove the tension and inflammation of them; by widening, as much as possibly can be, the capacity of the ureters, for the passage of these gritty concretions; for which purpose the injection of emollient clysters has a double advantage; both as they convey an internal fomentation to those slender tubes, by this warmth relaxing their contraction, and, by unloading the lower bowels of any accumulated fæces, they remove their lateral pressure against the ureters. To open yet more this passage, the warm bath proves of most important service: For, by this warmth and humidity, the abdominal muscles, peritonæum, and intestines, are greatly relieved from their tenseness; and the diminution of their former pressure allows a readier discharge of the gravel. Hence also the bladder is considerably relaxed; consequently the oblique insinuation of the ureters, thro' its several membranes, is less liable to obstruct the evacuation of this sandy matter into its cavity.

By moderate diuretics, by the uſe of oleagi-nous, emollient, and deterſive medicines, this diſcharge is greatly aſſiſted, and the paſſages themſelves at the ſame time properly lubricated, to facilitate the expulſion, while the ſeverity of pain is ſuſpended by anodynes, that produce alſo a kind of paralytic reſolution of the ſpaſmodic contraction of the fibres on the gravel (by which convulſive efforts to expel, they entirely preclude its deſcent); and therefore contribute not a little even to open the paſſage.

Theſe appear the moſt conſiderable methods for the relief of this diſorder, which is but imperfectly managed, without the united aſſiſtance of all; and which, uſed together, ſeem the utmoſt art can furniſh.

Let us now examine the methods of relieving theſe colics. By inſtant bleeding, the tenſion of the bowel is in ſome meaſure eaſed, a timely revulſion is made from the blood's impulſe on the affected part, an inflammation, with its conſequences, is prevented: By giving immediately briſk cathartics, we propoſe to urge forcibly forward the obſtructed, acrid, morbid matter; and, by the ſubſequent uſe of more lenient and leſs ſtimulating medicines, to aſſiſt the operation of the former with leſs violence; and, by the joint aſſiſtance of anodynes, not only to procure a ſuſpenſion of pain, but to diminiſh the convulſive contraction of the inteſtine. So far the curative intentions in either diſeaſe ſeem to quadrate with each other. The injections indeed of clyſters here are of little ſignificancy; for they cannot paſs beyond the valve of Tulpius, placed at the ilium's entrance into the cæcum; con-

ſequently their warmth cannot relax the affected inteſtine, nor their purgative quality diſlodge the obſtructed matter. Still the patient and phyſician continue alike diſappointed; the exceſs of pain remains, and the body, obſtinately coſtive, eludes the united effort of this method. The repetition of the more violent purgatives, by the ſtrength of their acrid ſtimulus, provokes a firmer tightneſs and contraction; and by this means they partly fruſtrate their own operation; yet the milder have already proved too weak to affect any thing. By enlarging the anodynes, his pain is but momentarily mitigated; and even theſe, by the coſtiveneſs they occaſion, are liable in ſome degree to prevent a diſcharge; yet the patient can never be ſafe, nor can the diſeaſe ever be cured, without procuring a thorough evacuation.

But what will be the conſequence of this obdurate obſtruction? Either certain death, or the contents of the ſmaller inteſtines, the medicines and the liquids the patient takes, if not returned by vomit, are together ſtopped at the contracted part, and, with the rarified air contained here likewiſe, dilate greatly the upper portion of the bowel to a very wide amplitude, while that below the contraction is empty, cloſe, and undiſtended; till, by violent ſtraining in the torture of this agony, the contracted part of the inteſtine is forced upward and inward within this widened part, too greatly ſtretched not to admit a ready introſuſception, one being ſo preternaturally ſtraitned, and the other immediately contiguous ſo vaſtly widened

beyond its natural dimenſion. This is the true, the formidable iliac paſſion, in which the veſſels ſurrounding the coats of the inteſtine are doubled, the circulation through them inſtantly prevented, and a ſwift mortification immediately follows. All which might certainly have been prevented, if the ſingle point of the inteſtinal ſtricture could have been removed, to make way for the evacuation; nor could the ſtrongeſt cathartics forcibly break through it. Should we not then attend to this point with great diligence, and apply to obtain it, a method ſo certainly ſerviceable in the nephritic caſe, by enlarging the ureters? Sydenham * indeed, in his diſcourſe on the iliac paſſion, which he ſuppoſes produced by the ſharp irritation of acrid matter, ſeems partly to propoſe relaxing the contracted bowel, by this method of applying an animal warmth to the pained part of the abdomen, laying a large puppy on it; though I confeſs his principal view appears to have been by this heat to ſtrengthen the weakened inteſtine, agreeable to what he lays down as the ſecond indication of cure; but he hints at no ſuch indication in his chapter on the bilious colic: Yet here only is it likely to be moſt advantageous, before the caſe has proceeded ſo far as to terminate in this dreadful ſymptom.

Shall we imitate this truly great man, and endeavour to improve on his method, by fomenting with emollient decoctions the pained part, and thus endeavour to direct and determine their effect to the very point where the ſeverity of pain

* Sect. 1. cap. 4. p. 41.

pain is complained of? But this will prove an insufficient, a defective, and a partial method, while an immersion in a warm bath, prepared of the same kind of decoction, is an universal fotus to the lower trunk of the body: Besides that, such a quantity of relaxing moisture cannot be imbibed by the bibulous vessels, from the expressed flannels, as must be absorbed from the surrounding fluid itself; where, used as a bath, the application to the part is the same, the warmth is equal, and the extent of its relaxing influence perfect and general on all the abdominal region. It may also be worth inquiry, whether the pressure of the water has not some superior influence to urge a larger proportion of fluid into those absorbent vessels.

We see then how nearly allied these two distinct and widely different diseases are in their respective curative indications; the grand article of relaxing, opening, and procuring a free passage being of equal importance in both. I dare not contend indeed, that the decoction of emollient plants actually carries any superior virtue in it beyond simple warm water; for the gentle heat and humidity of the latter may prove as sufficient for the purpose: Yet, as the worst that can be said of this artful disguise is, that it is a needless pomp, the *pia fraus* deserves to be continued; for the patient would be too apt to entertain a low, a thankless opinion of his physician's merit or depth, that effected his cure by so simple, so naked a remedy.

Upon the whole, I would not be apprehended to advance the notion, that no bilious colic can possibly be cured without warm bathing: The

The contrary is every day experienced; for, in the milder kind, where the inteftinal ftricture is far lefs ftraitened, the obftruction more readily yields to the force of cathartics. Yet, even here, in proportion to the abatement of this tightnefs, the more fpeedily will it affift in refolving it, the more conducive will it be to quicken the operation of internal medicines, to haften the patient's relief, and prevent any fubfequent danger; for which reafons I fhould not, even in fuch cafe, and at the beginning of the difeafe, omit it: And I am perfuaded, in the feverer kind, neceffity indifpenfably commands us to ufe fome method for relaxing the bowel, that an evacuation may be timely obtained; and cannot but believe many an iliac paffion might have been happily prevented by an early conjunction of the warm bath with the other manner of cure.

But it is juftly expected from every medical writer, that he fhould confirm and eftablifh, from experience, and the authority of repeated trials, the real efficacy of that method he recommends; for, unlefs the fanction of fact is on his fide, all the flourifh of laboured reafoning, and pomp of probable argument, is but ridiculoufly vain; a barren fuperfluity of words, a *vox & præteria nihil.* But this, though a neceffary, is yet a painful tafk to every modeft author; for even the fimple narration of fuccefsful truth is fo frequently conftrued a vain glorious boafting, and carries fuch an air of oftentation with it, that the cenforious world too often, fometimes too truly, concludes the hiftory was not fo much added to confirm the pre-

cee[d]ing

ceeding argument, as the argument designed to introduce the subsequent self-applauding story.

I shall however give some few instances, wherein I have experienced the success of this collateral aid of the warm bath: The first I produce is the case where I first used it for this purpose; and the happy instantaneous relief was, as far as a matter of this nature can be proved, evidently and entirely owing to it.

*July* 13. 1731.

A gentleman of Spittlefields, about thirty, of a moderate habit of body, by drinking adulterated bad wine in a journey, was immediately seized with a violent pain in the smaller bowels; which increased daily, attended with an entire costiveness: On his return, he immediately applied to his apothecary, who prudently attempted to remove the obstruction by various pertinent methods; as bleeding, and the stimulus of rougher cathartics: Which proving ineffectual, he tried the laxative method of more lenient medicines, a solution of manna in purging waters, attended with oily draughts; not omitting the proper use of opiates, to relieve the convulsive spasm of the intestine; he had also injected several clysters of the softer and the strongly irritating kind.——In vain. I found him in the utmost acute pain, as if a cord had been forcibly strained round the abdomen, attended with frequent vomiting of yellow, viscid, bilious matter; and attempted once more the effect of a smart cathartic, (*Extract. rudii drachm. sem. Calomel gr.* xv.) followed with a *decoct.* of *Fol. senn. and rad. rhei*, with the addition of

*Elix,*

*Elix. salutis* and *syr. ros. solutiv.* of which he took two spoonfuls every hour. I waited on him in the evening, but he had received no manner of benefit from this method, his torture continuing without any abatement, with the same inexpressible agony. I resolved therefore to try what might be the advantage of a warm bath, by relaxing the intestine, and opening the passage: One was instantly prepared from a decoction of the most emollient plants; and such was the happy consequence, that, even while he was in it, he had a loose stool, soon followed by five more, though he had but one for ten days before. This copious discharge terminated his disorder; the remaining tenderness of his bowels being soon relieved by an opiate, and a solution of *sperma ceti.*

*August* 28. 1731.

I visited———Eaton, a child not quite five years old, who was seized the 22d of the same month with a violent colicky pain immediately after eating two large raw codlings: Several clysters had been ineffectually thrown up, and several cathartics as fruitlessly taken internally; for the child had had as yet no stool: I found the abdomen greatly swelled and hard, with excessive pain; the pulse was frequent and strong; the respiration quick, laborious, and indeed struggling. Bleeding was immediately performed; and the blood drawn away was covered on its surface with a leathery substance, equally thick and hard with that of any adult, robust, pleuritic patient I had ever attended. He took instantly *pil. coch. min. gr.* viij. *ca-*

*lomel.*

*lomel. gr.* iii. in the form of pills; and within an hour began the use of the mixture following, ℞. *Fol. sennæ drach.* i. *Rad. rhei scrupul.* i. *Coque in aq. fontan. q. s. Colaturæ unc.* iii. *adde elix. salutis, mannæ an. unc. sem. M. Sumat. cochl.* ii. *omni hora donec responderit alvus.* A warm bath was prepared from a decoction of emollient plants; he drank also frequently a solution of *manna unc. sem. in unc.* iv. of *decoct. pectorale.*

In a few hours after twice using the semicupium, and taking a proportional quantity of the other medicines, the obstruction was happily removed, and five copious dejections concluded the disease; the greatest part of the apples returned crude and unaltered, with the stools.

*October* 3. 1732.

——— Casseck, a plumber about forty, was seized September 30th with a violent colic, soon aggravated to great intenseness of pain; either alternately fixed and contracted to a point, or strongly surroundiug the abdomen like a tightned girth, and had received no evacuation by several cathartics and two clysters administred to him. The keen severity of his pain extorted loud and constant screams from him. His pulse was strong and full; wherefore blood was immediately drawn, and a dose of *pil. coch. min. cum calomel.* prescribed: An hour after which, he entered on the use of the following medicine:

℞. *Fol. sen. drach.* iii. *Rad. rhei drach.* i. *Coque in aq. fontan. q. s. Colaturæ unc.* vi. *adde elixir salutis unc.* i. *sem. mannæ unc.* i. *Sal.*

*Glauberi*

*Glauberi unc. ſem. M. Sumat cochl.* iii. *poſt elapſam a pilulis horam, dein cochl.* ii. *omni hora.* He uſed the ſemicupium, made of emollient herbs, twice a-day, half an hour each time.

Fourth, His agony increaſed ſo violently laſt night, that five men could with difficulty overpower his ſtruggling, and detain him in bed: To quiet this ſeverity, I ventured on *gr.* i. *ſem.* of crude opium; by it his pain was greatly mitigated this morning, but without any ſtool. He continued this day in the conſtant repetition of the ſame opening mixture, and the bath, as before: Whence, the morning of the next day, he had five large ſtools, and with them an end of his miſery and diſorder. A ſolution of *ſperma ceti* finiſhed his cure, by removing the remaining ſoreneſs of his bowels.

*May* 3. 1733.

——— Lord, a plumber alſo by buſineſs, a robuſt man, about thirty-five, was taken with a ſtrong pain in the ſmaller inteſtines, April the 29th, that ſeemed like a tight bandage to gird the abdomen round. He was the patient of an apothecary, to whom I had frequently and ſtrenuouſly urged the uſe and neceſſity of warm bathing in ſevere colics: He had therefore from the beginning prudently joined this with the other method of ſtrong cathartics, but without any effect; I found his pulſe hard and ſtrong, his pain increaſed to perfect agony, either determined to a point, and piercing his body thro', or at other times binding forcibly the abdomen round. I attempted his relief in the following manner:

Extra-

*Extrahantur brachio ſanguinis unc.* xvi. *ſtatim.* ℞. *Extract. rudii ſcrup.* i. *Calomel. gr.* xv. *f. Pilulæ ſtatim ſumend. ſuperbibend. prorſus nihil: Tribus vero elapſ. horis, capiat coch.* ii. *miſturæ ſequentis.* ℞. *Fol. ſen. drach.* iii. *Rad. rhei drach.* i. *Sal. Glauberi unc. ſem. Coque in aq. fontan. q. ſ. Colaturæ unc.* vi. *adde elixir. ſalutis unc.* i. *ſem. Mannæ unc.* i. *M. Capiat cochl.* ii. *omni hora. Pergat in uſu ſemicupii.* This he uſed half an hour twice or thrice a day.

Fourth, As yet he found no laſting abatement of pain, though the bath conſtantly mitigated his agony, while he continued in it; nor was any ſtool obtained, though he had taken the whole of the opening mixture: Wherefore the following more quickening medicine was preſcribed: ℞. *Extract. rudii, calomel. an. ſcrup.* i. *Reſin. jalap. gr.* vi. *f. Pilulæ quamprimum ſumend.* ℞. *Fol. ſen. unc. ſem. Rad. rhei drach.* ii. *Sal. Glauberi drach.* vi. *Coque in aq. fontan. q. ſ. Colaturæ unc.* ix. *adde elixir. ſalutis unc.* ii. *ſem. Mannæ unc.* ii. *M. Capiat cochl.* ii. *poſt tres, ab aſſumptis pilulis, horas, & dein omni hora.*

He ſtill kept to warm bathing as before. I viſited him again in the evening, but found no benefit yet received, the inteſtinal obſtruction continuing equally obſtinate, with equal pain. His pulſe remaining ſtill full and hard, *unc.* xii. of blood were again drawn away, which, like the firſt, was greatly inflamed; he continued ſtill in the conſtant uſe of the aperient mixture. The next morning his body was happily opened; yet not before a conſumption of one and a

 half

half of the laſt preſcribed medicine: So difficult was it to get an open paſſage! Yet the following day our patient complaining of a pleuritic pain, and his pulſe continuing very hard, ſtrong, and full, his vein was opened a third time with ſucceſs.

I have thus honeſtly related a few caſes, wherein, I apprehend, the warm bath did conſiderable ſervice:- Its remarkable advantage in the firſt inſtance ſufficiently encouraged me to make repeated trials of its efficacy; and I am convinced it ever greatly aſſiſted and promoted the operation of cathartics, and greatly forwarded the patient's recovery.

I ſuppoſe it will ſcarcely be urged on the contrary, that it as evidently appears from moſt of theſe very hiſtories, that the bath was frequently repeated before the work could be performed; conſequently its virtue was of little avail, nor worth the trouble of preparing: But, where the obſtruction is great, where the tightneſs of the inteſtines is obſtinately firm, the ſtrength and repetition of cathartics muſt be proportional, the frequency and continuance of bathing muſt be kept to without wearineſs, that the virtue of this united method may at length prevail. In nephritic caſes, the bath, and all the other known methods of cure, muſt ſometimes be long perſevered in, before the ſabulous matter, lodged in the kidneys, or plugging up the ureters, can be brought away; becauſe, therefore, it has not an immediate effect here, we ſhall not raſhly pronounce it of no benefit: For, as in the inteſtines, the relaxing every

ſtricture,

ſtricture, and opening every obſtruction, will be earlier or later, as is the degree of their ſtrength, duration, and tightneſs.

On the other hand, I am far from attributing theſe recoveries ſingly to the bath, excluſive of the help from internal cathartics. But, when I conſider the very nature of this diſtemper, the manner in which it affects the inteſtine, the continuance of the obſtinate coſtiveneſs, the ſeverity of their torture, (ever mitigated by bathing), and the large quantity of cathartics of either kind taken before the paſſage was made, I cannot help concluding the ſemicupium was really and highly uſeful in their relief; greatly forwarding their cure, and preventive of worſe conſequences that would have ariſen from a continued unremoved obſtruction. In this ſingle view, of an aſſiſtant to the other methods of cure, I endeavour only to propoſe it.

XXVIII. *The Water of a Dropſy evacuated at the Navel; by Dr* FRANCIS PRINGLE, *late Preſident of the College of Phyſicians at* Edinburgh.

IN June 1726 I was deſired to viſit a woman betwixt forty and fifty years of age, who had laboured under a *hydrops aſcites* ſome years; and in that time had taken a great many medicines by the direction of a phyſician. The moſt powerful diuretics and hydragogue purgatives were given by another phyſician and me in vain, the bulk of her belly which was very great when I ſaw her firſt, rather increaſing;

ſing; ſo that at length, deſpairing of doing her ſervice by medicines, and ſhe refuſing to undergo the paracenteſis, we gave over preſcribing for her.

Having one day taken a pretty briſk purgative, ſhe told me that the waters ouzed at her navel: This ouzing continued conſtantly after, but was ſlow and gentle, giving her no other uneaſineſs than what was occaſioned by wetting her linens. She remained in this ſtate all the winter, without her belly increaſing, but her fleſh and ſtrength waſted.

In May 1727, having gone to Leith in a coach, in returning home, the orifices at the navel were ſo dilated that the water guſhed out in a ſtream, as if ſhe had been tapped; and with great difficulty ſhe was carried from the coach to her lodgings, where the waters continued to flow plentifully, while I was ſent for, the people about her computing that ſhe had diſcharged between 12 and 16 Scots pints of water. When I came, ſhe was ſo faint, her pulſe ſo ſunk, and her looks ſo ghaſtly, that I found it abſolutely neceſſary to put a ſtop to any farther diſcharge of water at that time, by proper compreſſes and bandage, and then ordered her to be laid in bed with her head low and her feet raiſed. She paſſed the night indifferently, but recovered her pulſe and ſpirits. Next morning, a chopin more of water ran out, when the wet dreſſings were changed for clean ones, and that day ſhe was ſeized with a fever, attended with violent and threatening ſymptoms, ſuch as vomiting, hiccough, and aſthma. The fever continued in this way

ſome

ſome days; but at length ſhe recovered entirely, and continued that ſummer and moſt part of autumn in a perfect ſtate of health. Her belly not being the leaſt ſwelled, and her fleſh, ſtrength, colour, and appetite being recovered.

In October, ſhe was ſeized all of a ſudden with a colic, attended with violent vomiting, and an obſtinate conſtipation, which cut her off in a few days.

Upon opening her body, there was no water nor any remarkable diſorder found in the abdomen, except that the uterus was vaſtly large, being ſchirrous all over, with its coats greatly thickned. It weighed four pounds and an half.

XXIX. *The* Menſtrua *regularly evacuated at an Ulcer of the Ancle; by Mr* JAMES CALDER junior, *Surgeon in* Glaſgow.

A Healthy, vigorous, labouring country girl, of an ordinary ſtature, ſtrained her right foot, at fifteen years of age, and again at nineteen, when a ſordid ulcer broke out in it: This being healed up in three weeks, ſhe ſoon after complained of a diſorder through all her body. At twenty years of age, her menſtrua appeared for the firſt time, but in very ſmall quantity.

The former diſorders ſtill continuing, ſhe was blooded at the *vena ſaphæna* of that right foot. Soon after which an ulcer was formed in that ancle, which has now continued above five years, a conſiderable ſhare of the *os calcis* having come out. This ulcer ſent out in two or

or three days of each month ... large a quantity of blood, as women generally pass in their courses, and this in regular periods, without any blooding at the intermediate time. Some days before this periodical hæmorrhage, she always complained of great pain in her foot, which became tolerably easy, as soon as the evacuation was past. She continued in this way till, in May 1733, the bones being cast out, and the ulcer beginning to heal up, while her body turned more plump and strong, the menses came the natural way, in much the same quantity, and continuing the same time they used to do at her foot, which bled none. She had a second natural return of the courses in June, but, in July, she passed the natural period, and her foot became more painful; but, the menses returning as in other women, the pain ceased; and she has ever since that time been in good health, with her menses regular in the natural way, without any other evacuation than of a small quantity of pus at the ulcer, which still continues a little open.

XXX. *An Account of Medical Discoveries, Improvements, and Books published in the year 1732, and omitted in the second volume of this Collection.*

*Discoveries and Improvements.*

SEveral anatomists, particularly Messrs Heister and Palfyn, having of late disputed, whether Steno's duct, between the nose and mouth, is

is pervious in the recent ſubject, as well as in the ſkeleton; Dr Kulm, profeſſor of medicine at Dantzic, affirms, that he has found and demonſtrated it to ſeveral to be pervious in a dear, a bear, wild goats, hares, calves, dogs, and in the human ſubject; and then mentions the manner of tracing it. He ſays there is a double opening of this duct into the mouth at the papilla, immediately behind the *dentes inciſores*, out at which orifices a liquor bubbles, when the palate is preſſed with the fingers from behind forwards, by which one is directed to introduce a hog's briſtle into theſe canals; and by puſhing the briſtle firſt perpendicularly, and then a little backward, it very eaſily paſſes into the noſe. Beſides, ſays he, the place of theſe ducts in the noſe may be readily diſtinguiſhed by its greater declivity, *Tabul. Anat.* XI.

Dr Pozzi, profeſſor at Bologna, endeavours to prove, in the Commentariolum tacked to his orations, *p.* 55. that the nails grow out from the tendons that are ſpread on the laſt phalanx of the fingers and toes.

*P.* 58. He ſays, That the thymus of a calf, ſoftened by maceration, diſcovers a lobe from which a milky liquor runs out when it is wounded; and, if air is afterwards blown into it, the whole thymus is diſtended, and may be dried, when it plainly appears to be compoſed of cells communicating with each other; upon the ſides of which muſcular fibres are to be ſeen. From which ſtructure he concludes, that the thymus in fœtuſes ſupplies the uſe of the lungs, ſerving as a receptacle for the chyle to be prepared in.

P.

P. 72. Our author relates the phenomena he remarked in diffecting a dog, whofe fpleen was cut out when he was very young. The liver, which was larger, and more heavy than ordinary, was alfo become more brittle. The *vena portarum* was enlarged. The gall-bladder was full of bile; the colour, confiftence, and tafte of which difcovered it to be more acrid than it is commonly. From thefe appearances, he is led to think the ufe of the fpleen to be for feparating a liquor like to fpittle, which may dilute the bile, and prevent its too great acrimony.

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A new Englifh difpenfatory, by James Alleyne, M. D. 8vo London.

Thomæ Fieni libri chirurgici duodecim de præcipuis artis chirurgicæ controverfiis, editio 2da, 4to Londini.

*Profodia chirurgica*; or a *memoria technica*, calculated for the ufe of old practitioners, as well as young ftudents in furgery; being a lexicon wherein all the terms of art are accounted for, their moft received fenfe given, and an exact definition of them from the beft Greek authors: Alfo their pronounciation as to quantity determined by proper marks over each fyllable. The fecond edition, 12mo, London.

Giufti-

Giustificazione di Antonio Benevoli, Cerusico e maestro delle insigne spedale di santa Maria nuova del citta di Firenze, dalle replicate accuse del Signior Pietro Paolo in Lucca, 4to, Florent.

Antonii Vallisneri opere mediche e fisiche, tom. 2. fol. Venet.

Nuove ed erudite osservazioni, storiche, mediche, e naturali del Dottor Girolamo Caspari, medico primario di Feltre, 8vo, Venet.

Saggio di medicina teorico practica di Carlo Gianello, 8vo, in Venezia.

Observationes de febribus praecipue intermittentibus, et ex earum genere continuis, deque earum periculis ac reversionibus praenoscendis et praecavendis per medelam tempestivam, efficacem, adaequatam, candide et perspicue propositam. Autore Paulo Gottlieb Werlhof, M. D. 4to, Hanover.

A discourse concerning fevers, in two letters to a young physician. The second edition, 8vo, London

Frederici Hoffmanni medicinae rationalis systematicae, tomi 4ti pars 2da, 4to, Halæ Magdeburg.

XXXI. *An Account of the most remarkable improvements and discoveries in Physic made or proposed since the beginning of the Year* 1733.

DR Boerhaave, professor of physic at Leyden, has communicated to the Royal Society at London, several accurate and very laborious experiments concerning mercury. The result of them may be summed up in these few articles.

1. Quick-

1. Quick-ſilver, however well purified, by paſſing through leather, waſhing, or diſtillations, repeated ever ſo oft, yields always a ſoft black powder of a ſharp braſſy taſte, when long expoſed to violent conquaſſation, or to a degree of heat about twice as great as that of animals.

2. Heat, near as ſtrong as what is neceſſary for diſtilling quick-ſilver, changes the greater part of mercury, if not all of it, into a heavy, ſhining, red, friable powder, of a veryſharp, metallic, nauſeous, penetrating taſte, which one can ſcarce free his mouth of, that long and violently diſorders the human body, and diſpoſes it to excretions.

3. The fluid quick-ſilver remaining after this red powder is ſeparated, is more fluid, and of leſs ſpecific weight than common mercury.

4. All the black, and very near the whole red powder, can be brought into the former fluid ſtate, by a more intenſe heat; and this revivified quick-ſilver enjoys all the properties of common mercury, and ſerves as well for repeating the ſame experiments.

5. The ſmall part of the red powder which is not revivified can ſcarce be raiſed by the force of fire, and becomes of a duſky colour, ſwelling like a ſponge, and vitrifies by the addition of ſome borax; but, when tried with lead, it flies off.

6. Quick-ſilver, when diſtilled by itſelf, or in water, requires a very ſtrong heat to raiſe it; but, if it is previouſly reduced to a black powder, by amalgamizing it with one fourth part of lead, and then is put among vinegar, the

the quick-silver rises before the vinegar boils, Philosoph. Transact. numb. 427. § 2.

In pag. 324. of vol. II. we mentioned quick-silver being the fashionable medicine at London; since which it has been given here at Edinburgh in several forms, different from those commonly in use before. Tho' several have taken an ounce or two of crude mercury each morning of several weeks, we know no instance of its increasing any of the sensible evacuations; but have been told, that some who used it thus had passed some of it with their urine, and that the hands of others under this medicine had gilded their snuff-boxes and heads of their canes.

Quick-silver rubbed violently with a double quantity of crabs-eyes, or of sugar candy, till it is extinguished, when it goes by the name of *mercurius alkalizatus*, or *Æthiops albus*, tho' it has been taken by some without any sensible effect, yet we have had the experience of a very small quantity of it having raised a high salivation to others.

Mercury, extinguished in prunells, has operated much in the same way with the *Æthiops albus*.

Quick-silver dissolved, by rubbing it strongly with any chemical oil, or with venice turpentine, has been given to the quantity of a scruple, half a drachm, or two scrupules in a day. Such pills keep some peoples belly open, others they purge, and a gentle ptyalism has sometimes been occasioned; nay, a high salivation has been raised by them.

Dr Plummer's æthiops, described in art. VI. vol. I. has been succesful in several desperate cutaneous

cutaneous diſeaſes, and obſtinate glandular ſwellings, when care has been taken by doſes adapted to the patients, and the proper uſe of gentle opiats, or purgatives to prevent its running off by ſtool, or its affecting the ſalivary glands too much.

An imitation of Belloſte's pills has alſo been tried; theſe are made by extinguiſhing quickſilver in reſin of guajac, then forming into pills, with a chemical oil, or with *ol. tartar.* and afterwards cruſting each of them over with the extract of rhubarb. Theſe purge gently. We have not heard that they increaſed the excretion of ſaliva.

Crude antimony is recommended in palſies, pains, and numbneſs that come on after a ſalivation, and is ſaid to have cured ſeveral who were paralytic from other cauſes. The method of giving it, is to begin with three grains, increaſing the doſe with three grains every day, till the patient takes half a drachm at once; after which the doſe is diminiſhed three grains every day, till it comes down to the quantity of the firſt doſe. *Commerc. Literar. Norimberg.* 1733. *hebdom.* 2. § 2.

The French phyſicians are at preſent much divided in their opinions concerning the medicine ſo famous at Paris in 1719 and 1720, under the name of the powder of the *Chartreux*, which is now generally named *kermes mineral.* It is the ſulphur of antimony prepared, by pouring a diſſolved fixt alkali ſalt; for example, *ol. tartari per deliquium* upon the powder of antimony; and, after digeſting them in a ſand heat for a day, to make the liquor boil two hours, when

when it becomes of a very red colour; and being poured off into another veſſel, lets fall a red powder, which is freed of the alkali ſalt, by waſhing it frequently with water; after which it is dried and kept for uſe. Half a grain, or a grain of this powder, given every three or four hours, produces no violent effects; but, by increaſing the doſe, it may be made to vomit, purge, and ſweat. Some commend it in their theſes and pamphlets, as the moſt univerſal reſolvent and deobſtruent, aſſuring us, that it almoſt infallibly cures pleuriſies, peripneumonies, aſthmas, catarrhs, anginæ, ſmall-pox, and many other diſeaſes. Others, on the contrary, are as poſitive, that it heats and thickens the blood, thereby increaſing obſtructions, and is particularly hurtful in all inflammatory diſeaſes. Unluckily both parties appeal to experience, and mention examples to ſupport what they argue for.

By the information we have received of the effects of the medicine upon which the great run is now at London, we can imagine it to be no other than a preparation of antimony, and that no milder one than the butter of antimony; than which there is ſcarce known a more violent deſtroyer of all animal ſubſtances, being compoſed of the reguline or metallic part of that mineral corroded by the concentrated acid of ſpirit of nitre, or of ſea-ſalt.

Dr Boeli at Brunſwick recommends the powder of the root of the valerian taken into the noſe by way of ſnuff, as an excellent reſtorer of weak ſight, and mentions ſome examples of his ſucceſs with it. As he orders it, there may be

be ſome doubt on what the ſucceſs depends. His receipt is, ℞. *Rad. valerian. fol. tobac.* ā *drach.* ii. *fiat pulv. ſubtiliſſim. Adde ol. deſtill. lavendul. Majoran. a. gutt.* iii. *M. Act. phyſico-medic. acad. natur. tom.* III *obſ.* 125.

Sir Hans Sloan, preſident of the royal ſociety, informs us of four children, who, after eating the ſeeds of the *hyoſciamus niger* C. B. or common henbane, were ſeized with great thirſt, ſwimmings of the head, dimneſs of ſight, ravings, and profound ſleep; which laſt continued two days and nights in one of them. He ſays the delirium occaſioned by theſe ſeeds, differs from the common, and in ſome meaſure agrees with that produced by the dutroa, a ſpecies of the ſtramonium, and by the bangue of Eaſt India, a ſort of hemp.

He cured all theſe children by bleeding, bliſtering in ſeveral places, and purging afterwards, with a medicine compoſed of *elect. lenitiv. ol. amygd. d. flor. ſulph.* and *ſyrup. pæon.* which operated both by vomit and ſtool.

The ſame gentleman alſo tells us of a quack, who cured the toothach, by conveying the ſmoak of burning henbane ſeeds, by means of a funnel, into the hollow tooth. Philoſoph. Tranſact. num. 429. § 1.

Dr Short, in his hiſtory of the mineral waters of Yorkſhire, Derbyſhire, and Lincolnſhire, has claſſed them into the warm, purging chalybeat, diuretic chalybeat, purging and plain ſulphur waters.

He found the warm waters to be impregnated with a mineral ſteam, vapour, or ſpirit, containing a moſt ſubtile and impalpable ſulphur, with

a

a calcarious earth, and fome nitre and fea-falt. Of thofe he mentions, that of Buxton feems to be the principal, the heat of which is equal in frofty weather to common river water with which two fifths of boiling water has been immediately mixed.

The purging chalybeat waters contain a mineral fpirit, fulphur, vitriol, nitre, and fea falt, with a calcarious earth, of which fome particles are attracted by the loadftone. Of thefe, Scarborough fpaw is now in greateft reputation; the principal falt of which is defcribed and painted by our author, as confifting of long cryftals made up of fix fides, which all concur at each extremity in forming a diamond point, and he calls it *nitre*.

The diuretic chalybeat waters confift of much the fame principles with the former clafs, only the falts are in lefs proportion. Of thefe there are great numbers in Yorkfhire.

The fulphur waters, befides fulphur, contain alfo marine falt and nitre, or nitre only and earth. Of thefe the ftrongeft is Harrowgate-well.

Dr Shaw, in his treatife on Scarborough waters, previoufly lays down a great many rules to be obferved in the inveftigation of all mineral waters, whereby we may be directed in our inquiries, and may be cautioned againft drawing conclufions too haftily, which deferve to be ferioufly confidered by all employed in phyfic.

The contents of the purging fpaw, which is what is chiefly ufed, are much the fame in Dr Shaw's account, as what we mentioned from Dr

Dr Short; only Dr Shaw has described the first salt, as consisting only of four sides, of which two are broad, and two are narrow; the two broad ones, which are opposite to each other, are extended into a sharp edge at one extremity of each crystal, the narrow sides not shooting out so far; and the reverse is observed at the other extremity of the crystal. He affirms this salt to be *sui generis*, and different from all the natural or artificial salts yet known.

This water proves a gentle cooling purgative; but, as the salt is in a small quantity, the operation of the water is often required to be assisted by some of the salt prepared by the apothecaries.

Dr Waltherus, professor at Leipsick, after comparing the muscles of the human body, with the descriptions of them published by several authors, particularly by Mr Winslow (in his *exposition anatomique*) has made several accurate remarks on them, which may serve as a supplement to the *anatome teneriorum musculorum repetita*, which he published formerly; but, as they will not admit of an abridgment, we must refer to the observations themselves, which are inserted in *Nov. act, erudit. mens. Jun.* 1733.

Dr Nichols, lecturer of anatomy at Oxford, has, in his *Compend. anatom.* contradicted the common received doctrine of the motion of the heart, and of the circulation of the blood, both in adults and fœtuses.

Prælect. XV. "The circulation of the blood "depends, (says he), on six motions, 1. Of "the right auricle. 2. Right venrticle. 3. Pul-"monary artery. 4. Left auricle. 5. Left "ventricle;

"ventricle; and, 6. Of the aorta. Of these, "the 1st, 3d, 5th are synchronous, or act "at the same time, as the 2d, 4th, 6th like- "wise do; but the 1st, 3d, 5th, are asyn- "chronous, or act at a different time from what "the 2d, 4th, 6th do; and therefore

| | | |
|---|---|---|
| The 2 auricles<br>The 2 ventricles<br>The 2 arteries | alternately | are relaxed,<br>are contracted. |

Praelect. XXIV. Our author has the following propositions concerning the circulation of the blood in foetuses.

"1. The blood of the ascending cava is fitter "for nutrition, muscular motion, and the sub- "tile secretions, than the blood that is carried "to the heart by the descending cava. For the "former consisting partly of blood lately tritu- "rated in the lungs of the mother, partly of "blood returning from the *vena portæ*, and de- "fecated in the liver, with the blood brought "back from the iliac and emulgent vessels, "may be looked on as arterious blood; the lat- "ter, on the contrary, being deprived of many of "its more subtile parts, bestowed on the nou- "rishment of the fibres, or in the secretions of "the brain, is altogether venous, and as it were "weak and poor, (*effætus*.)

"2. The ascending and descending aorta are "dilated and contracted at different times, or "have asynchronous motions.

"3 The blood of the ascending cava is push- "ed to the heart, at the time when the right "auricle is contracted, and the left auricle is "relaxed; and therefore it will not pass into the

 "right

"right auricle, and from that into the left; but "must go immediately from the cava into the "left auricle.

"4. The blood which is sent from the left "auricle, into the left ventricle, consisting "mostly of the blood of the ascending cava, is "wholly distributed to the heart and branches "of the ascending aorta.

"5. The blood which flows from the descend-"ing cava into the heart, passes partly through "the lungs into the left auricle, to be mixed "with the blood of the ascending cava, partly "passes into the descending aorta, not to be "mixed with the blood of the ascending artery; "that the blood which is returned to the mo-"ther may be venous, weak, and poor, (*effœtus.*)

"6. The *canalis arteriosus* being shut by re-"spiration, the descending artery acquires a "motion synchronous to that of the ascending "artery; and the blood of the ascending cava is "sent to the heart, at the time when the left "auricle is contracted, and the right auricle is "relaxed; and therefore is wholly poured into "the right ventricle, along with the blood of "the descending cava.

"7. The contents of the abdomen being pres-"sed by respiration, the umbilical arteries, um-"bilical vein, and the *ductus venosus* are soon "shut up.

"8. The usual crying of new-born infants "contributes much to the distension of the "lungs, and breaking down the particles of "the blood.

The Doctor next explains a figure he has caused to be drawn of the two auricles of the heart

heart of a fœtus to ſhew the canals, by one of which the *vena cava aſcendens* opens into the right, and by the other into the left auricle, at each of which he paints a valve.

He illuſtrates afterwards his ſcheme of the circulation of the blood in a fœtus by another figure, in explaining which he aſſumes the ſecond and third of the preceeding propoſitions as demonſtrated.

Dr Nichols's opinion concerning the circulation of the blood *in natis et non natis*, in born and unborn animals, is ſo different from what has prevailed ſince Harvey's time, that we cannot but wiſh he had been more explicit, and would add the experiments or other proofs that can be brought to ſupport his doctrine. With a view to be informed, and to induce perhaps the Doctor to explain himſelf more fully, we ſhall propoſe one queſtion which naturally offers itſelf, upon looking at his ſcheme of the circulation in a fœtus. What preſerves the form of canals to F, (the paſſage from the *cava aſcendens* into the right auricle), and to N, (the part of the aorta between the riſe of the left ſubclavian artery, and the inſertion of the *canalis arterioſus*), ſeeing, by the explication of the ſcheme, there are no liquors paſs through them ?

The cataract is generally now agreed to be for moſt part the cryſtalline humour rendered opaque, very few inſtances being brought of any membrane lodged in the poſterior chamber of the aqueous humour, and theſe have been ſuſpected to be no other than the membrane of the cryſtalline ſeparated. To confirm more the doctrine of the cryſtalline being the part affected

fected in the cataract, Dr Scheuckzer, physician at Zurich in Swisserland, communicates the dissection of two cataractous eyes, on one of which the operation had been performed with success. In the eye which had not been couched, the crystalline, which adhered to the circumference of the uvea, was of the colour of whitish pearl, but less bright. In the other eye, the crystalline was less, harder, inclined to a yellow colour, and depressed below the uvea. A cloud was observed in the part of the vitreous humour through which the needle had passed. *Act. physico-medic. acad. N. C tom.* III. *observ.* 36.

The cases in midwifery, mentioned by Mr Giffard, may be reduced to these two. 1. That the head of the child is fallen down among the bones of the mother's pelvis, and does not advance with her throws, and cannot be pushed back into the womb. 2. That a wrong posture of the child, the flooding or weakness of the mother, make it necessary to bring the child away by the feet.

In the former of these two cases, he made use of an extractor, which is so far different from that described in Art. XX. of this volume, that the blades of it consisted each of an oval ring bended, instead of an entire piece of thin steel. Dr Hody, the editor of Mr Giffard's book, publishes with it an improvement made on the extractor by Mr Freke surgeon to St Bartholomew's hospital, which seems to consist in one of the handles having a joint in the middle of it, and its extremity being a sharp hook or crotchet, which has a moveable flap to cover it, when it is not employed as a hook to operate with.

with. Mr Giffard obſerved ſometimes, that the *os uteri* gripped the child's head ſo firmly, that it could not be brought away with the extractor, till with his fingers he had dilated the ring which the womb made. As ſoon as he could catch hold of the head with his hands, he laid aſide the inſtrument. If, after the head was born, the child ſtuck at the ſhoulders, he endeavoured to make more way for them, by bringing out firſt one arm, and then the other; or, if that could not be done, he put a crooked finger into each arm-pit of the child, and ſo extracted it.

In the caſes where there is a neceſſity of bringing the child away by the feet, he dilates the *os tincæ* by ſpreading his fingers, puſhes back what parts are in his way, or ſlides his hand along them to ſearch for a leg: After he has brought this out, and tied a ligature upon it, he ſearches for the other, but is not anxious about finding it, if it is bended upwards on the child's belly, or the paſſage is wide enough by the woman's having formerly born children; for the leg he has brought out is ſufficient for bringing down the buttocks, till he can put his crooked finger, or the handle of one blade of the extractor armed with cotton in the groin, or a fillet put round the thigh; with any of which and the ligature on the other leg, he extracts the buttocks. When laying hold of the child's body, he turns it, if there is occaſion for it, as he ſays there commonly is in ſuch a caſe; becauſe the child has ſunk down in the womb without turning, ſo that its face is forward. When the ſhoulders are brought out, he

he brings down the arms to make more room; and then clapping one hand flat upon the breast of the child to ſupport it, he lays the other on the ſhoulders, and pulls. If the *os internum* has contracted round the neck, he dilates it: If the head ſticks notwithſtanding this, he advances the lower hand, till he can put a finger or two into the child's mouth; and then preſſing on the lower jaw, he draws with both hands, and ſeldom miſſes to bring the child away.

Our author took always care to put back the umbilical rope when it falls out before birth, and to diſengage it from the child's neck or body, or to cut it when it is twiſted round any of theſe parts, to prevent the placenta being pulled away, or the circulation being ſtopped in it.

He always ſeparated the placenta with his fingers, and did not pull it away by the navel-ſtring, and took care to free the womb afterwards of clotted blood and membranes, which, when left, occaſions pains and flooding. He is of opinion, no time ſhould be delayed in doing this after delivery, becauſe the womb contracts very faſt.

Clyſters to provoke too languid throws, an opiate to put away falſe pains, and to bring on the true ones, and a cordial draught after delivery when the woman was low, were all the medicines he uſed.

Mr Chapman, in his eſſay on the improvement of midwifery, p. 7. affirms, that he never did obſerve the lateral pointing of the womb, ſo much inſiſted on by Deventer, but has obſerved it turned forwards or backwards.

P. 10.

P. 10. He condemns greatly the ufe of the crotchet or hook in the delivery of women, unlefs when there is an entire certainty of the child being dead; which only can be determined by a concurrence of all the fymptoms mentioned by authors, and not by one or two of them.

P. 12. He condemns the make of the extractors he has feen others employ, but does not defcribe his own, nor his manner of flipping a fillet over the child's head.

P. 67. The moft effectual remedy in flooding is, according to him, to cover the patient's body with clothes dipped in oxycrate, repeating them as they grow warm, and giving cool acid liquors to drink.

P. 117. After a hard labour, and where there has been a neceffity of ufing fome violence, "the woman (fays Mr Chapman) is to be "treated as one bruifed by a fall. Here, as "a thing of the greateft fervice, I would re-"commend wrapping of the body round with "fheep's fkin haftily flea'd off, and applied as "warm as poffible: I have, for many years "paft, had a happy experience of this, and wifh "I had come fooner to the knowledge of it "than I did, as having always made ufe of it "with fuccefs." This method is recommended by Guillemeau.

Dr Schulze, profeffor of medicine at Hall, contradicts the common opinion concerning the umbilical veffels, in two differtations he publifhed, to be defended by his fcholars. He endeavours to prove in the firft, That the navel is not formed by the ligature which the midwives make,

make, or by animals gnawing the navel ſtring of their young with their teeth, but by nature; and that the umbilical veſſels ſeparate ſpontaneouſly, or with very little force, from the interior ſurface of the ſkin, in the ſame manner as we ſee the ſhrivelled navel-ſtring fall off from the exterior ſurface. After which the umbilical veſſels within a child's and other young creature's body contract and ſhrivel, their extremity by which they adhered to the navel becoming black and pointed, as if they had been burnt, and at laſt diſappear altogether, without leaving any veſtige that can be obſerved in the adult; for, ſays he, what are commonly deſcribed and painted, as theſe veſſels changed into ligaments, are no other than the ſheaths in which they were formerly contained.

It is eaſy to ſee what concluſion he draws from this doctrine in his ſecond diſſertation, viz. That it is unneceſſary to make any ligature upon the navel-ſtring after birth; but, as he judges the ligature to be innocent, he does not inſiſt upon its being omitted.

In confirmation of Dr Schulze's doctrine, Dr Eller, phyſician at Berlin, relates ſeveral inſtances of the navel-ſtrings of children being left untied after they were cut, without being attended with any hæmorrhage, or other bad conſequence. *Commerc. Norimberg.* 1733, *hebd.* 48. §. 2.

Dr Trew of Norimberg, having carefully examined the ſtate of the umbilical veſſels of ſubjects of different ages, obſerves that the ſhrivelling is not peculiar to theſe parts, ſeveral other parts

parts being changed in the ſame way, he could diſcover no mechaniſm by which a hæmorrhage ſhould be prevented without a ligature, when the umbilical veſſels are cut; and concludes, that, ſeeing there are examples of hæmorrhagies from neglecting to tie the navel-ſtring, it would be very unſafe to forbear the uſe of the ligature, *Ibid. hebd.* 49. § 1. & *hebd.* 50. § 1.

Dr Alexander Stewart, phyſician to the queen of England, formerly * communicated an obſervation of a ſerjeant of the horſe-guards, whoſe gall-bladder was wounded without any other bowel being much hurt, and whoſe ſymptoms, in the ſeven days he lived after being wounded, were a great diſtenſion of his belly, without ructus or flatus upwards or downwards, or borborygmi; no paſſage by ſtool, and very little urine, notwithſtanding purgatives and clyſters were given him, and that he took what was thought a ſufficient quantity of drink and liquid food; neither had he any ſound ſleep, but only ſhort ſlumbers, tho' he took opiates. There was no ſign of fever, his pulſe continuing in a natural ſtate till the day before he died, when it intermitted. The wound in the teguments never ſuppurated well.

After his death, the guts were found greatly diſtended, the gall-blader was almoſt quite empty, and a great quantity of bile was lodged in the cavity of the abdomen.

The doctor accounted for all theſe appearances, from the defect of bile within the inteſtines,



* Philoſoph. tranſact. numb. 414. § 2.

teſtines, to ſtimulate them to a due contraction, whereby they might reſiſt the elaſtic air, or might puſh the food or drink into the lacteals, or might expel their contents.——Their depriving him of a due recruit of chyle, occaſioned his want of ſleep, diminiſhed the ſecretion of urine, and prevented a ſuppuration in the wound.——The veſſels being emptied by the excretions that were not compenſated by a new chyle, while the more acrid particles of the blood were carried off in the ſecretion of bile, without any of them returning to the blood again; and therefore, the veſſels being neither ſtretched nor irritated, there could be no fever. ——Since there was a conſtant waſte of this man's liquors without any ſupply from the food, the doctor concludes him to have died famiſhed.

This account of the ſymptoms, however ingenious, did not fully ſatisfy ſome people, whoſe difficulties our author now endeavours to remove.

To thoſe Gentlemen who remark, that the gall (eſpecially in ſuch large quantity as in the hiſtory) when applied to the exterior ſurface of the guts, might have irritated ſome particular parts of them to a ſpaſmodic conſtriction, which would have occaſioned the diſtenſion of the intermediate parts, and the other ſymptoms, as well as the cauſes aſſigned above, the doctor replies, That nerves only exert their action at their extremities, where they are diveſted of their involving membranes; and therefore, in the caſe before us, the bile could not have given pain, nor excited a contraction in the muſ-

cular

cular fibres of the guts. 2. The irritation of the exterior membrane of the guts would have no effect on the muscular coat, because the former has its nerves from a source different from those of the latter. 3. The gall, being equally diffused over the surface of the guts, must have made an equal contraction of their fibres every where, which was not the case.

In explaining how a fresh recruit of chyle should be a cause of sleep, Dr Stewart is obliged to examine how and by what sleep is produced.

"I believe (says he) it will hardly be denied, "that the cause of sleep in general is a want of "a sufficient quantity of animal spirits," [for the existence of which he appeals to the arguments and experiments mentioned p. 326. of Vol. II.] "for the use and exercise of the animal func-"tions; therefore, whatever prevents their re-"cruit, hinders or impedes their secretion, ab-"sorbs or fetters them when produced; and "whatever exhausts and evaporates them, by "occasioning a paucity of spirits, will, in a "healthy person, produce a listlessness, laziness, "a tendency to sleep, or sleep itself, in propor-"tion to that pacuity of the remaining spirits." To illustrate this doctrine, he considers the remoter causes of sleep, which he reduces to four. 1. Exercise. 2. A too plentiful meal. 3. Drunkenness. 4. Narcotics. And then proceeds to shew how these produce such effects.

"Exercise wastes all the fluids, and particularly the animal spirits.——The great quantity of chyle, after a full meal, makes all the fluids of

of a thicker consistence, and absorbs the animal spirits.———Fermented liquors and ardent spirits, being observed to diminish remarkably the serous secretions, may therefore be reasonably concluded to fetter the finest fluid, which is the animal spirits, and to hinder it to be separated from the other liquors,——Soporifics act much in the same manner.

According to our author, in sleep, a sufficient quantity of spirits still remains for actuating the organs of the vital and natural functions, to which they are determined by more powerful causes, such is the impetuous blood in the heart, gravitating air in the lungs, digesting food in the stomach, &c. than there are to determine them to the organs of the animal functions. Hence, awaking is owing to a quantity of spirits so much greater than what is required for the vital and natural functions, that they must excite the animal likewise.

The last proposition which the Doctor undertakes to prove is, "That pus, being a gross secretion, is the product of the chyle, and not "of the blood or serum; for (says he) I think "that it would not be difficult to prove, that all "the gross secretions are from the chyle." *Philosoph. Transact. numb.* 427 § 2.

Mr Evan Davis communicates to the Royal society the histories of several children inoculated with the small pox at Haverford-West in Pembrokeshire, while the measles were epidemic there in February and March, after the small pox had been very mortal through the winter. Every one of them who were infected thus with the variolous matter, only became feverish in the

the ſeventh or eighth day after the inoculation, and the meaſles appeared ſoon after. On the twelfth day, they were again attacked with a fever, and on the fourteenth the ſmall pox, of a mild kind, were ſeen; *Ibid. numb* 429. § 9.

Dr Hobb, in his rational method of curing fevers, after having examined the nature of the fluids and ſolids of the human body, things neceſſary to health, (ſuch as, the air, aliments, ſecretions, and evacuations), the cauſes of diſeaſes, and particularly of fevers, concludes, p. 194, That, whatever may be the productive cauſes of fevers, the ſtate of the fluids in people under them muſt be one or other of theſe following:

1. That the animal fluids are too thick, that is, they are viſcous or glutinous, which ſeems to be the caſe in all inflammatory fevers. Or,

2. That they have particles too bulky for an eaſy circulation and excretion, till they are comminuted. Or,

3. That the animal fluids are too thin, and the natural union of their component particles is more or leſs diſſolved, and the globules of the blood and lymph more or leſs broken: Which appears to be the caſe in putrid, malignant, and many peſtilential fevers, and in all fevers attended with colliquative evacuations. Or,

4. That the fluids have acrimonious and corroding particles mixed with them, which is the caſe in all fevers attended with ulcerations from an internal cauſe.

While the fluids are thus affected, the ſolids are rigid or too dry; or they are too lax.

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It is impoſſible for us to mention, in the narrow bounds we are confined to, all our author's curative indications taken from the foregoing doctrine; we ſhall therefore do no more than remark, that he allows of blood-letting only to patients who have a ſanguine plethora, for the knowledge of which he refers to his treatiſe of the ſmall pox, where, as we mentioned in Vol. I. p. 286 he confines it to ſo many circumſtances, that it will ſeldom be met with; and in thoſe patients to whom letting of blood is proper, "the quantity of blood (ſays he, p 208.) "that may prudently be taken away at once, "I think, generally ſpeaking, ſhould not exceed "ſix or eight ounces; becauſe we cannot know "to an ounce or two the exceeding quantity, and "becauſe, if it ſhould afterwards appear, that "too little has been taken away, the remedy is "eaſy, by repeating phlebotomy; but, if too "much has been drawn off, it is not eaſy to redreſs the inconveniencies that may attend it" Afterwards, in conſidering the effects of blooding, Dr Lobb deduces from Mr Hales's experiments, that taking away ſix ounces and two drachms of blood from a man weighing 160 *lib.* may diminiſh the vital ſtrength, or the force of the action of the heart and arteries, about one tenth part. The loſs of twelve ounces and a half of blood will diminiſh his vital ſtrength above one ſixth part, which, ſays he, is a great diminution. Eighteen ounces and ſix drachms will diminiſh it above one fourth, which, he adds, is a vaſt diminution, and to be avoided, unleſs ſome extreme neceſſity ſhould require the taking

king of it. Thirty-ſeven ounces and a half of blood taken away, diminiſh the vital ſtrength of ſuch a man near one half, a diminution which ſurely ought to be dreaded. Fifty ounces may diminiſh it near ſix ſevenths; the conſequences of which may, ſays our author, be eaſily apprehended. From the whole, we think it may be concluded, that the doctor is not very fond of taking blood, eſpecially in a large quantity.

## XXXII. *A Liſt of Medical Books publiſhed ſince the Beginning of the Year* 1732.

A*Lbum Bavariæ Jatricæ, ſeu catalogus celebriorum aliquot medicorum, qui ſuis in Bavaria ſcriptis medicinam exornarunt, ab anno* 1450, *quo Boica ſchola fundata quidem, at primum anno* 1472 *publicata fuit, in hodiernam uſque lucem. Studio* Franc. Joſ. Grienwaldt, M. D. 8*vo*, Monachii 1733.

The treaſury of drugs unlocked; by Jo. Jacob Berlu, of London, merchant in drugs. The ſecond edition, 12*mo*, London 1734.

A treatiſe on the force and energy of crude mercury, by Thomas Harris, ſurgeon, 8*vo*, London 1734.

A critical diſſertation on the manner of the preparation of mercurial medicines, by T. K. M. D. 8*vo*, London 1734.

*Hampſtead-wells*; or directions for the drinking thoſe waters; with an appendix relating to the original of ſprings in general; with ſome experiments on the Hampſtead-waters, and hiſtories

ſtories of the cures, by John Soame, M. D. 8*vo*, London 1634.

An inquiry into the contents, virtues, and uſes of the Scarborough ſpaw waters, by Peter Shaw, M. D. 8*vo*, 1734.

An inquiry into the nature and principles of the ſpaw waters, by Charles Perry, M. D. London 1734.

*Toxicologia pathologico-medica, ſive de venenis, lib.* III. *Autore* Chriſtiano Godofr. Stentzelio, M. D. 4*to*, Vitemberg 1733.

Chemical lectures publicly read at London in the years 1731 and 1732, and ſince at Scarborough in 1733, for the improvement of arts, trades, and natural philoſophy, by Peter Shaw phyſician at Scarborough, 8*vo*, London 1734.

*Traité de chemie, contenant la maniere de preparer les remedes qui ſont le plus en uſage dans la pratique de la medicine. Par M. Maluin Docteur regent de la facultée de medicine de Paris*, 12mo, Paris 1734.

*Oſteographia;* or, the anatomy of the bones, by William Cheſelden ſurgeon to her Majeſty, *&c. fol.* London 1733.

The anatomy of the human bones, with an account of muſcular motion, and the circulation of the blood; alſo of digeſtion and nutrition, with a deſcription of the four ſenſes, illuſtrated with variety of copper-plates. To which is added, a ſhort and eaſy method of diſcovering the virtues of plants, in curing the diſeaſes of the human body; by George Thomſon, M. A. 8*vo*, London 1734.

*Spiramina*, or reſpiration reviewed; being chiefly the argument of that great philoſopher by

by fire, *Jo. Bapt. van Helmont;* discovering certain uses of the lungs not commonly observed, and asserting that they have not that alternate motion that is generally ascribed to them; but that in a sound man they are porous, pervious to the air, and constantly at rest, by M. J. 8vo, London 1733.

Compendium Anatomicum, *ea omnia complectens quæ ad cognitam humani corporis œconomiam spectant,* &c. *constructum, à* F. Nichols *é Coll. Oxon.* M. D. *Præl. Anat. Oxon.* &c. 4to, Londoni 1733.

Physical essays on the parts of the human body, and animal oeconomy, 8vo, London 1734.

*Anatomie chirurgicale de* Palfyn, *revuë, corrigée, et augmentée, accompagnée de notes dans le premier volume, et refondue dans le second, par M. B.* Boudon, *docteur en medicine. On y a joint les observations anatomiques et chirurgicales de Mr* Ruysch, *traduites du Latin, et celles de M.* Brisseau, *en 2 vol. 8vo,* Paris 1734.

The navy surgeon; or a practical system of surgery, by John Atkins surgeon, 12mo, London 1734.

Two hundred and twenty five cases in midwifery, which for the most part were attended with a great deal of danger and difficulty. Written by the late Mr William Giffard, surgeon and man-midwife, published by Edward Hody, M. D. 8vo, London 1733.

The art of nursing, the second edition, 8vo, London 1733.

An essay concerning blood-letting, by R. Butler, M. D. 8vo, London 1734.

*Suite*

*Suite des maladies chroniques, ou l'on traite celles qui arrivent à l'oeil, et des remedes les plus convenables pour les guerir sans operation manuelle, par P. V.* Dubois, *ancien Prevôt et garde des maitres chirurgiens de Paris, tom.* V. 12*mo*, Paris 1733.

*Nouvelles classes des maladies dans un ordre semblable à celui des botanistes, comprenant les gendres, et les especes de toutes les maladies, avec leurs signes et leurs indications; par* Sauvage de la Croix, *doćteur en medicine*, 12*mo*, à Avignon 1733.

A compleat treatise of the stone and gravel, by N. Robinson, M. D. the third edition, 8vo, London 1734.

*Meditationes theoretico-practicae de furore haemorrhoidum internarum, methodicè conscriptæ à* Justo Arnoldo Gulich, M. D. *editio altera*, 8*vo*, Lugd. Bat. 1733.

An essay on the gout, by T. Bennet, M. D. 8vo, London 1734.

Rational methods of curing fevers, deduced from the structure and oeconomy of human bodies, and the different states of the solids and fluids, under the different classes of fevers; by Theophilus Lobb, M. D. 8vo, London 1734.

The generous physician, or medicine made easy; by Dr Colbatch, 8vo, London 1733.

Frederici Hoffmanni *consultationum et responsorum medicinalium centuria prima, complectens morbos capitis et pectoris*, tom. I. 4to, Halæ Magdeburg 1734.

Michaelis Alberti *Acad. Frid. Prof. Med.* &c. *ulterior continuatio, aut tomus 3tius, jurisprudentiae medicæ*, 4*to*, Schneeberg. 1733.

Caroli

Caroli Musitani *jatrias prof. opera omnia. Edit. 2da, 2 vol. fol.* Lugdun. 1733.

The Philosophical Transactions (from the year 1720, to the year 1732) abridged and disposed under general heads, by Mr Reid and John Gray, F. R. S. 2 vol. 4to, London 1733.

The Philosophical Transactions (from the year 1719, to the year 1733) abridged and disposed under general heads, by Mr John Eames and Mr John Martyn, F. R. S. 2 vol. 4to, London 1734.

*Acta physico-medica academiæ Cæsareæ Leopoldino-Carolinæ naturæ curiosorum, exhibentia ephemerides sive observationes, historias, et experimenta a celeberrimis Germaniæ et exterarum regionum viris habita et communicata, singulari studio collecta. Volumen tertium, 4to,* Norimberg, 1733.

Philosophical Transactions for the year 1733, 4to, London.

*Commercium literarium Norimbergense anni* 1733. *semestr.* 2. *4to*, Norimberg.

*Dissertationes medicæ, quas, ex auctoritate amplissimi senatus academici Edinburgensis, et nobilissimæ in eadem academia facultatis medicæ decreto, pro gradu doctoratus, summisque in medicina honoribus et privilegiis rite ac legitime consequendis, examini subjecerunt.*

Joannes Lindesay Scotus, *De calore.*

Jacobus Grieve Scoto-Britannus, *De morbis humorum oculi.*

Carolus Ayton-Douglas Scot.-Brit. *De exercitationum in medicina usu.*

Joannes Arnot Scotus, *De abortu.*

Henricus Tonge Anglus, *De noxis ex cibi et potionis abusu oriundis.*

XXXIII.

## XXXIII. *Books proposed, and other medical News.*

DR Boerhaave, professor of medicine at Leyden, concludes the account of his experiments on mercury, with a promise, conditional indeed, but which probably the importunate requests of the learned will obtain, of publishing his experiments and remarks concerning the extraction of mercury out of metals, the action of mercury on metals, and concerning metals.

The new edition of the Edinburgh Dispensatory is now delivered to the printers, and will speedily be published.

Doctor Christ Jac. Trew of Norimberg has dispersed proposals for publishing a full, faithful, and distinct delineation and explication of all the parts of the human body. He does not propose to publish this whole work at once, but divided into sections. The osteology seems to be ready for the press; for the proposals inform us, That the bones of a young man are delineated in twelve tables in folio; the thirteenth plate represents the bones of the female which differ from the male. The conjunctions of the bones are to be explained in a plate larger than the others. The explication of these plates in high Dutch will be twelve sheet; and he promises this shall be translated into Latin or French, and printed, if any desire it.

Dr Thomas Simson professor of medicine in the university of St Andrew s designs to publish soon, a second edition of his system of the womb. The

The theorical part of which he is to enlarge considerably, and is to add a practical part, wherein he is to treat of the diseases of the womb, and their cure

Dr Lobb tells us, in his rational methods of curing fevers, That he has a treatise on the particular species of fevers near ready for the press, which he designs to publish soon, that he may complete his account of fevers.

Dr Michelotti, physician at Venice, has promised soon a treatise on the small pox.

Dr Grubert, physician at Brussels, is said to be about to publish a Medical Dictionary, in which there are to be several discoveries.

The subject proposed by the chirurgical academy at Paris, for gaining the prize of 1734, is, "To determine, in each kind of chirurgical "disease, the cases where it is fit to dress frequently, and those where it is fit to dress seldom."



*The End of the Third Volume.*

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